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*SECTION OF BIOLOGY**

SEMINAL VESICLE RESPONSE AS AN ASSESSMENT OF ANDROGENIC ACTIVITY†

By J. H. LEATHEM‡

The response of the rat seminal vesicle as a possible assay end point for androgenicity received considerable impetus from the reports of Hays and Mathieson[1] and Wills, Rampton, and Pugsley.[2] A maximal seminal vesicle weight increase was obtained 72 hours after an injection of 0.25 mg. of testosterone propionate in castrated rats. A seminal vesicle weight increase of the same degree can be induced in 22-day-old normal rats. The response is not influenced by the volume of the vehicle but is altered by the route of administration.[3]

Testosterone propionate will quickly increase the oxygen consumption, fructose content, water, and weight of the seminal vesicle of castrated rats.[4] In normal rats, we have found that 0.25 mg. of testosterone propionate increased seminal vesicle weight 214 per cent in 72 hours and increased water content from 78.0 to 82.0 per cent. This steroid will restore the alkaline phosphatase enzyme concentration to normal levels from the subnormal levels of the castrate. Both chemical and histochemical evidence support the androgen-enzyme relationship.[5, 6] In general, alkaline phosphatase enzyme in the rat seminal vesicle is concentrated in the subepithelial stroma, with little[7] or none[8] in the epithelium. After 3 hours' incubation in glycerophosphate, the normal 22-day-old rat gland exhibited some slight nuclear reaction in the epithelium, but the major concentration was seen in the subepithelial stroma. No change in enzyme distribution was observed 72 hours after injection of 0.25 mg. of testosterone propionate.

A definite weight response can be induced with 50 micrograms of the androgen in castrated rats.* We have studied three groups of 22-day-old normal rats after dosages of 25 and 50 micrograms given as a single subcutaneous injection. After 72 hours, seminal vesicle weight was 35, 54, and 87 per cent above control weight with 25 micrograms and 57, 105, and 110 per cent above control weight with 50 micrograms.

Since a 200 per cent seminal vesicle weight increase can be induced 72 hours after 0.25 mg. of testosterone propionate administration, it was of interest to test other androgens in the 22-day-old rat. Single subcutaneous injections of 0.25 mg. of testosterone, androsterone, dehydroisoandrosterone, epitestosterone, vinyl testosterone, androstanediol 3, 17 diacetate failed to induce a response, but testosterone hexahydrobenzoate did increase seminal vesicle weight 100 per cent. Increased dosages of varied androgens were studied to determine their relative androgenic action on the rat seminal vesicle, and these data are presented in TABLE 1.†

These data are of interest initially in comparison of testosterone, androsterone, and dehydroisoandrosterone. Parkes and Emmens[9] compiled a table of androgenic activity as gained from capon comb response and found that 15 micrograms of testosterone induced the same response as 100 micrograms of androsterone, whereas 200 to 300 micrograms of dehydroisoandrosterone was needed to duplicate the action. When these same androgens were studied in the rat, the seminal vesicle stimulating activity appeared to be virtually the same (TABLE 1).

* The Section of Geology and Mineralogy held a meeting on May 1, 1950, at which Matt Walton read a paper entitled "The Blashke Island Ultrabasic Complex, Southeastern Alaska." No abstract of this paper has been received.

† On May 8, 1950, the Section of Biology held a symposium, presided over by W. W. Scott of the Johns Hopkins University School of Medicine, on "Recent Studies on the Mammalian Male Sexual Accessory Glands." This paper, illustrated with lantern slides, was the first of four presented at the symposium.

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* This investigation was supported in part by a grant from E. R. Squibb & Sons to the Protein Metabolism Fund of the Bureau of Biological Research, Rutgers University.

† The androgens were generously supplied by Ciba Pharmaceutical Products, Summit, New Jersey.

Kochakian[10] found that the percentage increase in combined seminal vesicle and prostate weight was the same 30 days after pellet implantation of androsterone and dehydroisoandrosterone but that testosterone was much more active.

With the exception of testosterone, more general agreement was observed in comparing capon comb and seminal vesicle weight response. In the capon, the following androgens are listed in descending order of their activity: testosterone, methyl-dihydrotestosterone, dihydrotestosterone, methyl testosterone, androsterone, dehydroisoandrosterone and cis-testosterone.[9] In the rat, methyl-testosterone may be slightly more active than 17 methyl-dihydrotestosterone and both were more active than dihydrotestosterone. Vinyl testosterone was more active than testosterone which, at best, was little more active than androsterone and dehydroisoandrosterone. Epitestosterone has little activity in the capon, and no response was obtained with 3.5 mg. in rats. However, esterification clearly increased the activity of this steroid.

TABLE 1

SEMINAL VESICLE WEIGHT RESPONSE OF 22-DAY-OLD RATS 72 HOURS AFTER A SINGLE SUBCUTANEOUS STEROID INJECTION

Steroid	*Maximum dose injn. mg.*	*Sem. ves. increase %*
Testosterone prop.	0.25	200+
Testosterone hexahydro-benzoate	0.25	100
Methyl testosterone	0.75	63
17 Methyl dihydrotestosterone	1.0	43
	2.0	133
	3.5	215
Dihydrotestosterone	1.0	48
	3.5	93
	5.0	192
Vinyl testosterone	2.5	60
	5.0	100
Testosterone	3.5	70
Androsterone	3.5	60
Dehydroisoandrosterone	3.5	60
Epitestosterone	3.5	0
Epitestosterone prop.	2.5	27
	3.5	93
2 Bromo-testosterone hexahydro-benzoate	2.5	0

Summary. An increase in seminal vesicle weight of the immature rat is readily obtained 72 hours after a single subcutaneous injection of testosterone propionate. Water content of the glands is increased, but histochemical alkaline phosphatase concentration is not changed. Assessment of other androgens permits some estimation of comparative androgen activities and indicates that differences exist between capon comb and seminal vesicle responses.

References

1. HAYS, H. W. & D. R. MATHIESON. 1945. Effect of testosterone propionate on seminal vesicles of juvenile and castrated male rats. Endocrinol. **37:** 266.
2. WILLS, C. G., S. E. RAMPTON, & L. I. PUGSLEY. 1949. Variables affecting the assay of testosterone propionate using the seminal vesicle response of the juvenile castrated male rat. Endocrinol. **44:** 251.
3. LEATHEM, J. H. 1948. Volume of oil and route of administration as factors influencing testosterone propionate activity. Proc. Soc. Exper. Biol. & Med. **68:** 92.
4. RUDOLPH, G. G. & L. T. SAMUELS. 1949. Early effects of testosterone propionate on the seminal vesicles of castrated rats. Endocrinol. **44:** 190.
5. STAFFORD, R. O., I. N. RUBINSTEIN, & R. K. MEYER. 1949. Effect of testosterone propionate on phosphatases in the seminal vesicle and prostate of the rat. Proc. Soc. Exper. Biol. & Med. **71:** 353.
6. ATKINSON, W. B. 1948. Effect of androgen on alkaline phosphatase in the seminal vesicle of the mouse. Anat. Rec. **100:** 731.
7. DEMPSEY, E. W., R. O. GREEP, & H. W. DEANE. 1949. Changes in the distribution and concentration of alkaline phosphatases in tissues of the rat after hypophysectomy or gonadectomy, and after replacement therapy. Endocrinol. **44:** 88.
8. BERN, H. A. 1949. The distribution of alkaline phosphatase in the genital tract of male mammals. Anat. Rec. **104:** 361.
9. PARKES, A. S. & C. W. EMMENS. 1944. Effect of androgens and estrogens on birds. Vitamins & Hormones **2:** 361.
10. KOCHAKIAN, C. D. 1947. The role of hydrolytic enzymes in some of the metabolic activities of steroid hormones. Recent Progress in Hormone Research **1:** 177.

THE RESPONSE OF THE MALE MOUSE SEX ACCESSORIES TO TESTOSTERONE DURING INANITION*,†

By E. D. GOLDSMITH‡ AND R. F. NIGRELLI§

A number of investigators have demonstrated that optimum growth in several groups of animals requires adequate supplies of pteroylglutamic acid (PGA). Folic acid has been shown to be essential for metamorphosis[1] and growth[2] of several species of insects. Goldsmith and Harnly[3] have reported that both a crude antagonist (X methyl PGA, Lederle) and an antagonist of known chemical structure, 4-amino PGA (Aminopterin, Lederle), prevented the development of the larvae of the fruit fly, *Drosophila melanogaster*, into adult flies. The inhibitory effects have been overcome with large amounts of PGA and much smaller quantities of desoxyribonucleic acid.[4] Chicks and monkeys maintained on a diet deficient in PGA showed a decreased growth response of the female genital tract to estrogens.[5] A PGA deficiency produced by the administration of PGA analogues has resulted in a reduced response to female sex hormone of the oviducts of the rat,[6] chick,[6, 7] and frog.[8, 9]

Since PGA is apparently required by diverse tissues for their optimum growth, it was intriguing to explore the growth reactions of the sexual accessory glands of testosterone-treated castrate male mice maintained on a ration containing a PGA antagonist. Extensive experiments revealed that the seminal vesicle-coagulating gland complex of the castrate mice fed a Purina chow diet containing either X methyl PGA (Lederle)[10] or 4-amino-N[10]-methyl PGA (Amethopterin, Lederle)[11] did not grow as well as did those in the mice on the control stock diet. Pretreatment with and administration of large quantities of PGA concurrently with the Amethopterin negated the action of the antagonist.

Although the evidence in favor of the view that folic acid was required for the satisfactory growth response of the male accessories to the male sex hormone appeared ample, the considerable weight loss of the antagonist-treated mice made it necessary to ascertain whether the observed reduced growth response of the accessories of this group to testosterone might be due not to a lack of folic acid *per se* but rather to the concomitant inanition. It is true that it had been reported previously that inanition in the rat[12, 13] did not alter the potentiality of the sex accessories to respond to either gonadotrophin or testosterone. Additional evidence supporting this finding is to be found in the work of Mason and Wolfe,[14] and Werner.[15] Studies on the chick,[16] guinea pig,[17] and man[18] have also yielded similar results. Nevertheless, as a further check, it was considered desirable to investigate the reaction of the accessory sexual glands of the castrate mouse to testosterone during inanition.

Twenty-eight to thirty-three day-old CFW mice were castrated and were placed on a Purina chow ration for 20 days. The animals were then divided into 3 groups: (1) and (3) were fed *ad libitum* (approximately 4 gm. per day); (2) was restricted to 1 gm. per day. A fourth group was composed of normal non-castrates of the same age.

In TABLE 1, the results of a typical experiment are tabulated. After 7 days, the mice in group 2 had lost about ¼ of their total body weight. At this time, daily parenteral administration of 1 mg. of testosterone propionate* in 0.04 cc. of sesame oil or an equivalent quantity of sesame alone was initiated. All animals

* On May 8, 1950, the Section of Biology held a symposium, presided over by W. W. Scott of the Johns Hopkins University School of Medicine, on "Recent Studies on the Mammalian Male Sexual Accessory Glands." This paper, illustrated with lantern slides, was the second of four presented at the symposium.

† This investigation was supported in part by a research grant from the National Cancer Institute of the National Institutes of Health, U. S. Public Health Service.

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* The testosterone propionate (Perandren) was generously supplied by Dr. F. F. Yonkman and Dr. F. L. Mohr of Ciba Pharmaceutical Products.

TABLE 1

UNDERFEEDING AND THE RESPONSE OF SEXUAL ACCESSORIES OF CASTRATE MALE MICE TO TESTOSTERONE

Group*	No. survivors per group	Procedure	Orig. body wt. (gm.)	Final body wt. (gm.)	Accessory wt. (mg.)	Accessory wt. (mg.) / 100 gms. body wt.
1	14	Fed *ad lib.* 11 days. 1 mg. testosterone injected daily last 5 days	18.9	23.2	55.3	237.7 ±42.2†
2	30	Fed ½ *ad lib.* 11 days. 1 mg. testosterone injected daily last 5 days.	21.4	13.9	14.1	102.2 ±18.7
3	8	Fed *ad lib.* 11 days. Sesame oil injected daily last 5 days.	18.0	23.5	6.4	29.6 ± 3.0
4	5	Fed *ad lib.* No treatment	21.7	24.1	205.3	865.5 ±73.1

* Groups 1, 2, and 3 composed of animals castrated at 28–33 days of age. Experimental treatment instituted 20 days after castration. Group 4 mice were normal non-castrates.
† Average deviation.

were sacrificed 24 hours after the fifth injection.

Seminal vesicles, coagulating glands, and prostates were removed, weighed as a unit on the torsion balance, and prepared for histological examination. When the experiment was terminated, 50 per cent of the mice in group 2 had died and the survivors had lost about ⅓ of their initial body weight. Both the fatalities and weight loss were comparable to those observed in animals treated with Amethopterin during a similar period.

Examination of the table reveals that both on an absolute and on a 100 gm. per body weight basis the accessory genitalia of the mice on the restricted diet were significantly lighter than those fed *ad libitum*. Histological examination demonstrated that the seminal vesicles were juvenile in appearance and contained no secretion. The glandular epithelial cells were lowest in the inanition group. The periurethral glands, which serve as an excellent indicator of testosterone activity, were absent in these animals.

It would thus appear that the response of the accessory sex organs in the castrate mouse to a sufficient supply of male sex hormone is governed by an adequate food intake. It is hoped that completion of experimental work, now in progress, utilizing purified diets deficient in folic acid or pyridoxine, and the treatment of starved animals with the several factors of the B complex, alone and in combination, may further clarify the situation.

References

1. GOLDBERG, L., B. DE MEILLON, & M. LAVOIPIERRE. 1944. Nature **154:** 608.
2. FRAENKEL, G. 1950. Physiol. Zool. **23:** 92.
3. GOLDSMITH, E. D., E. B. TOBIAS, & M. H. HARNLY. 1948. Anat. Rec. **101:** 93.
4. GOLDSMITH, E. D. & M. H. HARNLY. 1950. Cancer Research **10:** 220.
5. HERTZ, R. 1948. Recent Progress in Hormone Research, II: 161. Academic Press.
6. HERTZ, R. & W. W. TULLNER. 1949. Endocrinology **44:** 278.
7. FRANKLIN, A. L., D. LEWIS, E. L. R. STOKSTAD, & T. H. JUKES. 1948. Poultry Science **27:** 662.
8. GOLDSMITH, E. D., S. S. SCHREIBER, & R. F. NIGRELLI. 1948. Proc. Soc. Exp. Biol. Med. **69:** 299.
9. GOLDSMITH, E. D., S. S. SCHREIBER, & R. F. NIGRELLI. 1949. Proc. Soc. Exp. Biol. Med. **71:** 461.
10. GOLDSMITH, E. D., H. M. BLACK, & R. F. NIGRELLI. 1949. Nature **164:** 62.
11. GOLDSMITH, E. D., H. M. BLACK, & R. F. NIGRELLI. 1949. J. Clin. Endocrinology **9:** 674.
12. MOORE, C. R. & L. T. SAMUELS. 1931. Am. J. Physiol. **98:** 278.
13. MULINOS, M. G. & L. POMERANTZ. 1941. Endocrinology **29:** 267.
14. MASON, K. E. & J. M. WOLFE. 1930. Anat. Rec. **45:** 232.
15. WERNER, S. 1939. Proc. Soc. Exp. Biol. Med. **41:** 101.
16. BRENEMAN, W. R. 1941. Endocrinology **28:** 946.
17. STEPHENS, D. J. & W. M. ALLEN. 1941. Endocrinology **28:** 580.
18. STEPHENS, D. J. 1941. J. Clin. Endocrinology **1:** 257.

THE INFLUENCE OF CERTAIN STEROIDS ON THE BEHAVIOR OF HUMAN PROSTATIC CANCER*

By J. B. TRUNNELL AND B. J. DUFFY, JR.†

Ten years have passed since Huggins' and Herbst[2] first demonstrated the spectacular improvement which follows castration or estrogen therapy in patients with cancer of the prostate gland. The decade, however, has been one of sharp contrast between bright hope and keen disappointment. In a recent review, "Chemotherapy of Malignant Disease," Gellhorn and Jones[3] have said, "to date androgen control therapy of prostate gland cancer has provided the single, most outstanding, practical contribution to the chemotherapy of malignancy." A consideration of the five-year survival data in a large series,[4] however, calls forth real doubt that these measures significantly prolong life, a fact which only heightens the need for investigation in this fallow field.

Failure to understand the *modus operandi* of control in these cases hinders one in comprehending the now crucial problem of relapse. Several hypotheses have nevertheless been advanced to explain in general terms these closely integrated phenomena. The most popular explanation is that advanced by Huggins.[5] This investigator postulated that the removal of androgen producing tissue or the peripheral neutralization of androgens deprives prostatic tumor cells of a substance essential to their prosperity. He explained relapse in terms of either (1) increased production of adrenal androgen, or (2) the development of "androgen independence." A few patients in relapse have been subjected to removal or irradiation of the hypophysis or of the adrenal glands. The present impracticability of such surgical procedures and the inconsistency of the radiation results led us to search for more physiologic means of inhibiting androgen production.

Albright[6] has proposed that androgen production by both the adrenals and the testes is largely controlled by the luteinizing or interstitial cell-stimulating hormone (LH or ICSH). It has long been believed that the output of a pituitary trophic hormone is inhibited by the hormone of the gland which it stimulates. In accordance with this principle, androgens and progesterone are both thought to depress the output of LH in normal endocrine physiology. Exogenous androgen or progesterone might therefore be expected to reduce the availability of LH and, secondarily, to reduce the elaboration of endogenous androgens.

In the interest of keeping the total (exogenous and endogenous) androgen at a minimum, progesterone, though slightly androgenic, was selected for initial clinical trial.* This compound was administered in doses of 25–300 mg. daily, either as an aqueous suspension or dissolved in oil. Fifteen patients have been so treated. Eight of these were previously untreated, 7 responding well. The single treatment failure, subsequently, also was not benefited by castration. Seven other patients were in spontaneous relapse following remission induced by castration or by estrogen therapy. Six of these also responded favorably to progesterone administration. One or more patients showed disappearance of pain, return of appetite, weight gain, correction of anemia, relief of urinary obstructive symptoms, return to gainful employment, and lowering of the serum acid phosphatase levels, accompanied by an alkaline phosphatase flare.† Whether the reasoning which led to its trial was correct, these experiments demonstrated progesterone to be an effective therapeutic agent in the treatment of prostatic cancer.

A troublesome pharmacologic problem

* On May 8, 1950, the Section of Biology held a symposium, presided over by W. W. Scott of the Johns Hopkins University School of Medicine, on "Recent Studies on the Mammalian Male Sexual Accessory Glands." This paper, illustrated with lantern slides, was the third of four presented at the symposium. A fourth paper, entitled "Further Hormonal Relationships in Prostatic Growth," was presented by Doctor Scott. No abstract of this paper has been received.

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* Supplied through the courtesy of Dr. E. C. Reifenstein, Jr., of Ayerst, McKenna, and Harrison, Ltd., and Dr. Edward Henderson of the Schering Corporation.

† During initial stages of favorable response, patients with bone metastases often show a rise in alkaline phosphatase before the level of this enzyme begins to fall. This is thought to be a function of bone regrowth in perimetastatic areas.

arose, however, in that the majority of patients developed painful induration at the site of intramuscular injection. In an attempt to resolve this difficulty, other routes of administration were employed. Taken orally, progesterone is relatively inactive. In the female, however, pregneninolone (anhydro-hydroxy-progesterone or ethinyltestosterone) by mouth is an effective progestational agent. Under the latter circumstances, it is felt to possess only about one-tenth the activity of parenterally administered progesterone. It was in the use of pregneninolone that we first observed evidence of pluripotential response. Several patients improved dramatically on oral pregneninolone therapy (25–100 mg. per day), whereas other patients receiving similar doses grew rapidly worse, with rises in the serum acid and alkaline phosphatases accompanying clinical deterioration. There seemed little doubt that pregneninolone had a variable effect on prostatic cancer.

A similar dual-effect potentiality was then observed in a patient receiving progesterone parenterally. He had been controlled with 200 mg. per day (intramuscularly in oil), but, when a total of 1500 mg. progesterone pellets was implanted subcutaneously, he became rapidly worse. Pain returned almost at once, and the phosphatase values rose to very high levels. On a dosage-response basis, pellets, because of constant, slow absorption, are much more effective than single daily injections. It appeared, therefore, that in this patient, whose cancer had been suppressed at one dosage level of progesterone, there was unquestionable stimulation at a higher level.

Having observed this behavior in two compounds, we began to wonder if the same tendency might be observable in the use of estrogen or of testosterone. The latter substance has often been said to stimulate the growth of prostatic cancer to a marked degree. As mentioned, however, testosterone should theoretically be capable of decreasing endogenous androgen production, and, if this result could be achieved with small quantities, the total (exogenous and endogenous) androgen level might be lowered sufficiently to inhibit tumor growth.

We were rather surprised to find that only 6 out of 21 patients could be made to exhibit clear-cut evidence of tumor stimulation when given testosterone. In fact, one patient received 100 mg. testosterone propionate daily for one month without stimulating effect. Indeed, Brendler[7] has reported testosterone therapy of 3 patients with prostatic cancer with clinical improvement. Some of our patients, as well, have claimed to feel better following injection of small doses (10–15 mg.) of testosterone, but we have seen no corresponding alteration in the serum acid phosphatase level. The latter failure may be attributed to the fact that patients so studied in our clinic were seen at weekly intervals. Hence, it is possible that transient lowering of phosphatase values occurred but were not measured. It may be mentioned in passing that those patients who did exhibit sensitivity to testosterone (were made worse) showed an improved response to estrogens following such stimulation. Taken as a whole, these observations on the influence of testosterone do not convincingly demonstrate the divergent capabilities (stimulation and control) clearly shown following treatment with either progesterone or pregneninolone. They suggest, nevertheless, that under appropriate circumstances such demonstration may be forthcoming.

It has been even more difficult to obtain evidence that, at high dosage levels, estrogenic substances might also stimulate prostatic cancer activity. In fact, at least one group of investigators[8] has failed to observe stimulation with doses of 500 mg. of stilbestrol per day over periods of several months. Scott,[9] on the other hand, has seen increased pain and elevation of acid phosphatase values in a patient given estrogens. These changes were reversed when estrogen administration was discontinued. We have observed similar changes in a

patient who received 680 mg. estrone* intravenously together with 96 mg. ethinyl estradiol orally during an 8-day period. He was subsequently brought under control with progesterone.

Seyle[10] has called attention to a "periodic table of the steroids" in which, like the chemical elements, he has arranged certain steroid hormones according to their biological activity. Our experience with four steroid hormones (namely, estrogen, progesterone, pregneninolone, and testosterone) has led us to feel that, in the order named, these substances may be similarly arranged, and that further, probably as a function of dose size, prostatic cancer appears to be either controlled or stimulated by each compound. In other words, following administration of the compounds named above, eight responses are predictable. Actually, we have observed six of these responses and even have some inconclusive, but suggestive, data concerning the remaining two (stimulation with large doses of estrogens and inhibition with small doses of testosterone).

The foregoing concept, which is considerably less rigid than the older "androgen neutralizes estrogen" idea, serves as a starting point for possible explanation of some heretofore perplexing aspects of therapy of this disease. In order not to be prematurely lured into serious consideration of these explanations, however, we must remain cognizant that an imposing number of other variables exists. In mentioning only the more obvious of these, we may list competence of hepatic functions; duration and extent of the cancer; presence or absence of the testes; and, perhaps most important of all, the availability of certain essential nutritional factors. In the latter connection, we have been intrigued by reports of Hertz[11] and others, who have shown that folic (pteroylglutamic) acid deficiency will not permit estrogen to exert its usual growth stimulating influence on the chick oviduct. These workers have shown that the estrogen effect may be prevented equally well by a folic acid-deficient diet or by a folic acid antagonist. In either instance, the administration of folic acid restores incompletely the ability of the oviduct tissue to respond to estrogen. We have twice observed in one patient that the folic acid antagonist, a-methopterin (4-amino-n-methylfolic acid) apparently deprived estrogen of its ability to control prostatic cancer. This man was fairly well controlled with estrogen but had slight elevation of acid phosphatase. After 14 days of a-methopterin (5 mg. per day orally), he exhibited a sharp increase both in pain and in phosphatase values. These subsided after stopping administration of the antagonist and were subsequently reproduced during a second course of its administration.

The obvious implication of these results is that folic acid may be required for optimum therapeutic results with estrogen. We have, however, been unable in two instances of spontaneous relapse on estrogen to restore a state of control by administration of even large doses (250 mg. per day) of folic acid. These patients claimed to feel better, but their claims were not supported by decreases in acid phosphatase levels. Hertz,[11] on the other hand, was also unable to obtain complete restoration of activity (oviduct response) by the addition of folic acid to a "purified" diet. He postulated that one or more other substances might have to be supplied in addition. His observations suggest that pantothenic acid, riboflavin, and pyridoxin are *not* specifically required for estrogen action, but that liver contains agents capable of restoring full estrogen reactivity of the oviduct. We are currently investigating the influence of liver extracts on the quality and duration of estrogen induced remissions.

In conclusion, it must be confessed that whatever light our observations have shed upon this fascinating problem has served only to illuminate its enormous complexity.

* Premarin solution supplied by Dr. E. C. Reifenstein, Jr., of Ayerst, McKenna, and Harrison, Ltd.

References

1. HUGGINS, C. & C. V. HODGES. 1941. Studies on prostatic cancer. I. The effect of castration, of estrogen and of androgen injection on serum phosphatases in metastatic carcinoma of the prostate. Cancer Research, **1:** 293.
2. HERBST, W. P. 1941. The effect of estradiol diproprionate and diethyl stilbestrol on malignant prostatic tissue. Tr. Am. A. Genito-Urin. Surgeons **34:** 195.
3. GELLHORN, A. & L. O. JONES. 1949. Chemotherapy of malignant disease. Am. J. Med. **6:** 188.
4. American Association of Genito-Urinary Surgeons. Collected data.
5. HUGGINS, C. 1946–47. The prostatic secretion. The Harvey Lectures: 148.
6. ALBRIGHT, F. & H. ELRICK. 1948. An attempt to classify hormonal disorders of the hypophysis. Trans. Assoc. Am. Phys. **61:** 41.
7. BRENDLER, H., W. E. CHASE, & W. W. SCOTT. Prostatic Cancer: Further investigation of hormonal relationships. Arch. Surg. In press.
8. DRAPER, J. W. 2nd Division (Cornell) Urological Service, Bellevue Hospital, New York. Personal communication.
9. SCOTT, W. W. Personal communication.
10. SELYE, H. 1947. Textbook of Endocrinology. 1st Edition: 62. Acta Endocrinologica. Univ. de Montreal, Montreal, Canada.
11. HERTZ, R. 1945. The quantitative relationship between stilbestrol response and dietary "folic acid" in the chick. Endo. **37:** 1.

SECTION OF PSYCHOLOGY

PSYCHOLOGY IN MODERN MANAGEMENT*

By MARION A. BILLS†

What are the functions of an industrial psychologist? As I see them, they are (1) to use the special knowledge which our training as psychologists has given us to advance better management; (2) to increase that store of knowledge; and (3) to make available to qualified users the results of our increase in knowledge.

In what fields of management should the psychologist work? The study of psychology is the study of humans. Therefore, any phase of management which involves people is a legitimate field in which we as psychologists may function: employee selection, job evaluation, merit rating, time and motion study, incentives, machine design, training, promotion, counseling, job satisfaction, wages, vision, recreational activities, communications, costs, *etc.* Every one can add from his own experience other items to this list. This wide range of possible activities has been both a handicap and an advantage. The physician, the engineer, or the lawyer have their responsibilities better defined. Specialists in their field are recognized. Twenty-five years ago, the industrial psychologist had to function both as a general practitioner and a specialist, for we had no specialist to whom to turn. Today, this is less true. The industrial psychologist today functions in the capacity of a general practitioner, and, as it is for any practitioner, it is our duty to diagnose the case, conscientiously work out the treatment (using specialists, when necessary), and sell that treatment to management.

Some examples from my own experience may illustrate how it seems to me a general practitioner should work. First, a problem on which we have been working for many years, and intensively now for several months: the better selection of life insurance agents. In meeting this problem, our first question was: have there been any methods already worked out which have stood the test of time, or must we produce something new? As a whole, psychologists have been less inclined than the medical or engineering profession to use the information that we have and more inclined to want to try something different. Perhaps this is the natural fault of a young profession. Until we learn to draw to the fullest extent on our backlog of knowledge, however, we will not become a mature profession.

* This paper was presented at the meeting of the Section, May 15, 1950.

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In the selection of life insurance agents, the literature showed that two methods of selection had worked with reasonable success, Strong's Interest Test, scored for life insurance, and a weighted application blank. The next question was: will these methods predict success in our particular firm? We decided two years ago that we would make an intensive study of Strong's Interest Test. The Life Insurance Agency Management Association was making a study of the weighted application blank, and we knew that we could trust their results. In 1948, we gave the Strong's Interest Test to all full-time agents hired. By the end of 1949, the "A" men had an average production of 3.5 times the low-scoring group, and 53 per cent were still employed. The "B+" men had an average production of 2.1 times the low-scoring group, and 43 per cent were still employed. Of the low scoring group, only 21 per cent were still employed.

This data convinced us that the employment of "A" men was definitely better management, but, in the life insurance business, one must sell not only an Agency Department, but agencies and managers, spread all over the United States and Canada. All of this was interesting data to them, but it was simply data. How should we say it to bring it home to them? Here is the way we stated it. If an agency wanted from its new men a million-dollar production by the end of the following year, they would need to hire, in a given year, on the average of 4.8 persons scoring "A," 7.8 persons scoring "B+," or 16.7 persons scoring lower. This is just a re-arrangement of the data, but it is something that can be remembered easily. The picture becomes vivid to the manager. He sees 16 men for whom he will have to provide office space (and rents are high), a desk, and chairs. He sees 16 men come and go in and out of his agency. He must educate them, train them, counsel them on how they are using the training, listen to their disappointments and troubles, give them pep talks, re-educate them, and, in some instances, give added financial assistance. He also sees too large a proportion of these men being unsuccessful and he realizes his responsibility to society in not encouraging men to go into the business whose chances of success are low. When he really thinks this out, and the figure of 16 men helps him to do it, he tends to vote for the 4.8 better men instead.

The Life Agency Management Association had, meanwhile, made a detailed study, in which we cooperated, of a weighted application blank. Adding this information to our study, we can say to the manager, "If you want a million-dollar production by the end of the following year, you will need to hire 4.3 men scoring high in both Strong's Interest Blank and the weighted application blank, but you will need to hire 22.9 men scoring low in both. That is, you will have to hire five times as many." We feel that this is as well as we can do at present with objective means; it is as well as most tests predict. Our next step is to turn to some of the more subjective selection approach.

First, the interview. In testing this, we have had experience. Each year, we hire a number of casualty special representatives who, after training in our home office schools, enter our agencies as liason between our commission agents and the agency. In 1946–47, these men were being hired by our managers in the field, and each man interviewed was rated by the interviewer on 10 qualities chosen as essential to the job. A year later, the managers rated the individuals on their success as special representatives. In 1948, a study of these two ratings showed no relationship. Of the 150 men studied, however, 41 had been interviewed by a home office representative who had wide experience in interviewing, and for these there was a definite positive relationship between his interview rating and the rating of the manager at the end of a year. Based in part on these results, we changed our procedure. In 1949, no special representatives were hired without an interview by an experienced home office representative. This was costly, of course,

but in 1949 our experience was sufficiently better, so we are continuing the new procedure. It was feasible for the small number of special representatives which we hire. It is not feasible for the large number of life agents whom we contract each year. Therefore, it becomes a problem of training our 150 to 200 managers and supervisors in interviewing methods.

One probably thinks immediately of the patterned interview. At present, we use a patterned interview for the selection of our life agents. It has, I think, been useful for two reasons. First, it outlines in detail information that must be obtained. That information is valuable, and, in getting it, we slow down the interview so that more time is spent with each applicant. But, while the patterned interview tells the interviewer what information he should get, it does not train him in the methods of getting that information and in the subtle means of interpreting that information. The full technique can come only by experience, but experience itself does not make a good interviewer. Bad habits are so easy to acquire in interviewing that persons often become increasingly poorer interviewers with experience, especially when there is no check on interviewing results. We are struggling at present with the idea of typical transcribed interviews to which our supervisors may listen and give their criticisms and their reactions, which can then be directly checked with actual results.

A second source of subjective-selection information is recommendations. Although I have no conclusive results as yet, I will describe our present approach to this subject, since it illustrates another way in which the industrial psychologists may work in helping to organize material already in the minds of management (let me add here that many business executives are very good psychologists).

After a meeting at which the value of recommendations was discussed, one of the sales managers present said that he felt one could interpret a recommendation by the adjectives used, even though on the surface they all seemed good. This to him was simply a hunch, and, if it was to be useful for others, it had to be systematized.

We started the study by reading over several hundred recommendations. One of these recommendations still sticks in my mind. It was a neighbor trying very hard to recommend a young college man, but trying also to be honest. After several paragraphs of factual matters, he came up with, and you could almost hear a sigh of relief, a positive statement, "He is a good dresser." From the study of these recommendations, we drew off 20 words which were often used—all favorable. With the 20 favorable items listed, we then asked a group of our supervisors each to choose 10 people, 5 whom they considered good salesmen and 5 not so good, and check a blank for each of them. We headed our list "Please indicate, by underlining, the 5 adjectives which you feel describe this individual most accurately. Please underline 5 only."

In our sample run, we found a difference in the items checked for the successful agent and the unsuccessful one. Of course, certain of the qualities were neutral. For a year now, we have attached this form to our requests for recommendations and a year from now we hope to have definite information on it.

Not all of an industrial psychologist's problems are in the selection field, so I will choose as a second illustration a problem in a different field. As is true for all companies, not only do we want our company to be a good place to work, but we want our community to recognize us as such. What makes a job desirable? Most of the literature on the subject has been from manufacturing concerns. We felt that a clerical organization might give different answers. Also, we wanted the opinion of our own employees. We asked a fair sampling of them to fill out a blank, headed "What Makes One Job More Desirable Than Another." It listed fifteen items, taken mostly from other surveys. It was not an attitude survey. We did not ask

the individual, for example, whether he considered that the policies of our company were good or bad, but only whether he considered them important. The importance of the items came in order: "a fair boss," "work which interests one," "opportunity for getting ahead," "good pay," and "security."

Since "good boss" got the first place, the questions arose, "what makes a good boss" and were our bosses filling the bill? For an answer to this, we turned to a specialist. For two summers, we had a person trained in interviewing come in and interview employees chosen at random. The topic of the interview was how they liked their job, but the boss came into practically every discussion, and we believe the opinions expressed were quite frank and uninhibited, since they were given to an outsider and their highly confidential nature was stressed. The third summer, our specialist interviewed our supervisors. The information of the first two summers was given to our supervisors in generalized terms, and, by talking it out, they were able to see its application in detail. We believe that our supervision has improved.

A few of the items that came out regarding what makes a good boss may be of interest. A good boss expects results, but he doesn't insist on being the only boss. He lets you boss your own job. He doesn't check progress too often. He isn't always peering over your shoulder. He gives praise in public when due; criticism in private. He takes your requests seriously, even though he has to turn them down. He sees that the work is explained to you, and he tells you, or at least lets you know, whether it is good or bad.

Perhaps the most pleasing result of this survey was the fact that a majority of our employees wanted to be the boss of their jobs. They wanted to make at least the minor decisions themselves. That is the American way. This desire to boss one's own job is one which should be kept alive in our world of industry, and anything which we as psychologists can do to further it is more than worth the effort. I want to digress a minute, but it is really simply to stress this point.

A little more than 25 years ago, the Ætna Life Affiliated Companies realized that they had grown too large for the miscellaneous salary structure, which was influenced largely by each supervisor's own opinion, and decided in fairness to all employees to work out a job evaluation. We used a classification system based largely on complexity of work which had been developed by a group at the Bureau of Personnel Research at Carnegie Institute of Technology. The choice of a system was less difficult then than it would be now. Point evaluation and factor weighing systems were yet to come into the picture. This classification method seemed to fit our needs and, with minor changes, is the one we use now for our entire home office staff. In general, it breaks down into three groups of jobs, with several steps in each group. First, there are the jobs where there are only a few rules and regulations to follow and where all of these rules and regulations are definite and specific. The second group is where there are a large number of rules and regulations to be followed in carrying on the work, but, again, these rules and regulations are definite and specific. The third group are the decision-making jobs. To those who think of a large office as made up of a number of standardized jobs where people add figures, file cards, or check reports, it may come as a surprise that 45 per cent of our jobs are decision-making ones. Let me give you a typical example of such a job. Its title is Life Underwriter. The man holding this job must make his decision, from the papers submitted by the agent to the home office, whether or not to accept the individual as a risk. That is, this man, on his own responsibility, can and does commit the company. For this group, we do not need to worry that they are experienced in making their own decisions.

Probably at least one-third of the persons holding a job where rules have to be followed are headed for decision-making jobs

They are learning to make decisions. This leaves us approximately 35 per cent where the supervisors must take special pains to see that the individual realizes what is true, that he is the boss of his own job. The ways of doing this are numerous. Let me quote one out of the many which we use: measured production with pay for meeting or exceeding the accepted minimum established. For example, the woman who has turned out 1500 lines of typing in a day, instead of the minimum of let us say 1000, has bossed her own job and has budgeted her own time; she is proud of it and she is paid for it. She is not, and realizes that she is not, simply a cog in the wheel.

I would like to give you an illustration in the field of clinical psychology. I am not a clinical psychologist and I make no pretense of being one. As the only psychologist in an organization which did not have a psychiatrist until recent years, however, mental and emotional maladjustments which present themselves in any firm gravitated to my sphere. The first problem was to convince management that mental disease should be viewed in the same way as a heart attack or tuberculosis. Not all cases recover, but many do. The second problem was to hunt for reliable outside sources and, through them, to train our supervisory group to recognize early stages of mental and emotional maladjustments, and to use to the full these outside resources for the help of individuals in need. Help can come from psychiatrists, clinical psychologists, ministers, priests, clinics, and such organizations as Alcoholics Anonymous. We used them all at times and found them most willing to cooperate. We still use them, though three years ago we met the problem head-on and added a psychiatrist to our medical staff.

Following the advice of the old colored preacher who, in describing his sermon said, "I tells them what I'm going to tell them, then I tells them, and then I tells them what I told them," I want to review the points which, directly or indirectly, I have tried to make. First, as industrial psychologists, it is our duty to study and use all of the backlog of knowledge which our training has given us, but to use it only after we have proven its application to our own particular problem. Second, any definite bit of research takes time and involves two selling processes to management: (1) the selling for cooperation in trying the experiment and (2) selling the results, so that they will be used. We have become so used to talking of correlations, standard deviations, and so forth, that we sometimes forget that to the layman these terms mean nothing. It takes imagination to restate our findings, but it pays.

Finally, the company's industrial psychologist may solve problems by application of his own knowledge and that gained from published material. He may call in a specialist for temporary help, or he may advocate the employment of a specialist when need seems to demand it. All have their place.

To sum it all up, we are general practitioners in the field of psychology.

SECTION OF PHYSICS AND CHEMISTRY

THE ROLE OF IONIC DEFORMATION IN SURFACE CHEMISTRY*

By W. A. WEYL†

Surfaces and interfaces are the seat of asymmetrical arrangements of ions. In these strong electrical fields, one has to expect that ions change their electron density distribution, which, in turn, must cause them to assume chemical properties which differ from those of the same ions in

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, May 16, 1950.

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the symmetrical environment of their aqueous solutions. The polarized or deformed ion has an electron density probability distribution which has a lower symmetry than that of the gaseous or of the hydrated ion. One can express its state of polarization in terms of the character of the binding forces as well as in terms of the formation of a dipole or of a multipole. These descriptions can be useful in many ways, but they do not lend themselves directly to a chemical interpretation.

This paper represents an attempt to describe an adsorbed, strongly deformed ion in terms which indicate that its chemical behavior has assumed directional properties.

A cupric ion is polarized when adsorbed at a metal. The polarizability of an ion represents its ability to adjust its electron distribution probability and, with it, its own force field in order to suit best the force field of the environment. One may expect, therefore, that the deformation of the electron cloud of the Cu^{2+} ion is such as to lead towards the establishing of a "metallic bond" in the direction of the metallic phase and towards increasing the strength of its "ionic bond" in the direction of the ionic solution. This type of deformation can also be described as an inner disproportionation of the Cu^{2+} ion which, in its extreme, would cause one-half of the Cu^{2+} ion to become metallic copper (higher electron density than that of the Cu^{2+} ion). The other half, having lower than average electron density, would then correspond to a copper ion with a fourfold positive charge. This description explains the greater tendency of the deformed ion to attract electrons from an electron donor. The combination metallic copper-cupric oxide is reduced more readily by hydrogen than the pure oxide, free of copper. Since the reaction leads to metallic copper and thus is producing its own catalyst, however, the reduction of CuO has an induction period and is autocatalytic.

Similar descriptions of the state of polarization of adsorbed ions can be given for carrier surfaces other than metals. An attempt is made to present a few simple rules which describe the shift in chemical behavior if an ion is transferred from its aqueous solution to an interface. Catalytic phenomena explained on this basis can be easily correlated with physical phenomena such as wettability and hygroscopicity of solids, semiconductor properties of thin metal films, or the increased electron emission of the oxide coated cathode.

Introduction. The parameters used for describing a crystal as an infinitely extending network—namely, symmetry, coordination number, and internuclear distance or bond length—are not valid for the crystal surface. The coordination of surface ions is necessarily incomplete and the internuclear distances are different from those of the bulk, because surface ions are exposed to one-sided attraction. In the past, the problem of the surface structure of crystals and its deviation from that of the bulk has been approached primarily from a physical-mathematical viewpoint. The early discussions of Born, Madelung, Lennard-Jones, and their associates are well known. More recently, Verwey[1] in Holland and Stranski and Moliere[2] in Germany have contributed to this problem.

It is known that, as soon as the polarizability of the ions exceeds a certain value, one has to expect the formation of a special structure in the outermost surface planes of an ionic crystal. Ions of low polarizability prefer positions of highest symmetry and lowest electrical field strength. In a NaCl crystal, for example, the Na^+ ions are symmetrically surrounded by the anions. Ions of high polarizability can be deformed, however, to such an extent that the gain in deformation energy is more than sufficient to compensate the Coulomb repulsion between ions having electric charges of the same sign. In the mercurous chloride, for example, the highly polarizable Hg^+ ions occupy positions between the anions and other Hg^+ ions. Such a crystal has a lower symmetry, and the cations are not in the positions of lowest electrical field strength but in the electrical field

between Hg^+ and Cl^- ions. In a strong electrical field, the concept of ionic size loses its significance. In a highly symmetrical environment, *i.e.*, in a crystal or in a solution, it is justifiable to assign to each ion a certain "radius," because the repulsion force between two ions approaching each other increases very strongly as soon as a certain critical distance is reached. For all practical purposes, a closer approach becomes impossible and it is, therefore, justifiable to treat the ions as rigid spheres. This size of the ion loses its meaning if the ion is transferred into an asymmetrical environment, a fact which has been emphasized by Fajans[3] in his discussion of the internuclear distances of thallous compounds as compared with those of the alkalies. The identical cell dimensions of thallous perchlorate and rubidium perchlorate indicate that the Tl^+ and the Rb^+ ions have identical sizes when compared in symmetrical force fields. As a result of the one-sided attraction within the vaporized molecule, however, the internuclear distance in TlCl (2.55 A.U.) is much smaller than in RbCl (2.89 A.U.) and only a little larger than in NaCl (2.51 A.U.). In the thallous iodide, the distance (2.87 A.U.) is even smaller than in sodium iodide (2.90 A.U.). This mutual deformation of ions in asymmetrical force fields has been described also in terms of the character of the binding forces, and one says that the thallous halides are more covalent than the corresponding alkali salts.

For the purpose of interpreting the electrical properties of polarized substances, however, the degree of covalency is of minor concern, and it is preferable to describe the deformed ions as electrically distorted units having dipoles. For example, Roberts[4] used this approach in studying the electrical properties of alkali halides, and for this purpose it proved to be most helpful.

The deformation of ions in the outermost surface plane can be expressed in terms of dipole or multipole formation, as well as in terms of the character of the binding forces. In the last few years, we have made laboratory examinations of deformation of the ions at crystal surfaces by means of their characteristic chemical reactions. For this purpose, the rules derived by Fajans[5] were applied, and it was found advantageous to describe a polarized or a deformed ion in terms which indicate that, with its deformation, not only its electron density distribution, but also its chemical properties, have become asymmetrical and directional.

Since the development of modern crystal chemistry, through the work of Goldschmidt,[6] it has been known that the role which an ion plays in a crystal structure is determined by its size, charge, and polarization properties. Later workers in this field, however, overemphasized the size and the charge because these parameters lend themselves to an easy numerical description. As a result, the polarizability of an ion, one of its major properties, has not been sufficiently considered.

At the present time, Frankenburg, Komarewsky, and Rideal[7] are bringing out a series of books on "Advances in Catalysis." In the preface of the first volume they write: "a purely empirical approach is still the only safe way to search for efficient catalysts. . . ." It is true that one cannot predict quantitatively the catalytic activity of a substance, but it will be shown in this paper that there is a fair chance to understand and even to predict catalytic phenomena, at least in a qualitative fashion.

In the same preface, these authors write: "In other words, a science of catalysis has to be erected on foundations which still have to be laid." It seems, however, that the foundations for the science of catalysis are very clearly defined.

Heterogeneous catalysis is "chemistry in asymmetrical force fields" and, as such, it can be understood if one knows how the electrical field affects the chemical properties of an ion. It is the polarizability which describes the response of ions to electrical fields. This property has been neglected for too long.

It is the object of this paper to discuss surface properties of two extreme groups:

one having cations of the non-noble gas-type with very high polarizability, such as Hg^{2+}, Pb^{2+}, *etc.* and the other having small cations of the noble gas-type with low polarizability, such as Si^{4+}. The proton having no electrons has zero polarizability and, consequently, H_2O represents an extreme case of the second group.

Polarization Properties of Ions. Our knowledge of the polarization properties of ions is based primarily on the interaction between their electron clouds and the alternating electrical field of light. The response of the outer, more loosely bound electrons is by far the most important contribution to this interaction, so that the optical properties of an ion or molecule, in particular their refractivities, are closely related to their chemical properties.

The concept that the outer electrons of an atom or ion do not move in rigid orbits, but are affected by their immediate environment, goes back to Haber, Reis, Langmuir, Lewis, and others. A systematic treatment of polarization phenomena and the inter-relation which exists between polarization properties and physical or crystal-chemical properties can be found in the publications of Fajans and his students, Grimm and Sommerfeld, as well as Goldschmidt. Especially, Fajans[5] and his students evaluated the deviations from additivity of the polarizabilities in respect to the interaction of ions when forming compounds or crystals. These relations are often referred to as "Fajans' Rules," and the most pertinent ones will be mentioned here.

(1) An anion is larger than a cation with the same number of electrons. With increasing positive charge, a nucleus exerts stronger attraction forces upon its electron cloud. As a result of this "tightening" of the electrons, their response to the electromagnetic field of light decreases. The polarizability decreases in the following order:

$$O^{2-} > F^- > Ne > Na^+ > Mg^{2+} > Al^{3+} > Si^{4+}.$$

(2) A similar tightening of the electrons of an anion can be observed if the anion combines with protons. The polarizability of Cl^- ions is much greater than that of HCl. The polarizability decreases in the following series:

$$O^{2-} > OH^- > H_2O > H_3O^+.$$

(3) A tightening of the electrons of an anion is also noticeable if it is brought into the positive force field of cations. This effect increases (a) with the size (polarizability) of the anion, for example, in the following order:

$$F^- < Cl^- < Br^- < I^- \text{ or } O^{2-} < S^{2-} < Se^{2-};$$

(b) with decreasing size of the cation—the polarizing power of cations increases in the order:

$$Cs^+ < Rb^+ < K^+ < Na^+ < Li^+ \text{ or}$$

$$Ba^{2+} < Sr^{2+} < Ca^{2+} < Mg^{2+};$$

and (c) with increasing positive charge of the cation

$$Mg^{2+} < Al^{3+} < Si^{4+} < P^{5+}.$$

(4) Anions exert a similar, but in most cases a weaker, influence upon the cations. Cations having the same electronic structures are smaller and they are less polarizable, because their corresponding orbits are much closer to their nuclei and are, therefore, already tightened by their positive force fields.

(5) The mutual deformation of ions is at a minimum for ions of the noble gas-type. It is greater for ions having a complete 18 shell and reaches its highest value for ions with incomplete outer orbitals. The polarizing power and the polarizability increase from Sr^{2+} (8) to Cd^{2+} (18) and are greatest for Pb^{2+} (18 + 2).

(6) The mutual deformation is at a minimum in crystals of high symmetry. It reaches its maximum in the asymmetrical force field of the vaporized molecule or in the surface.

This last statement concerning the polarization or deformation of ions in asymmetrical force fields makes it imperative to the

chemist interested in heterogeneous catalysis to pay attention to the polarization properties of ions. It is true that one can go relatively far in the understanding of crystals and in the interpretation of their properties without using the concept of polarization if one restricts oneself to (a) crystals of high symmetry; (b) crystals consisting of noble gas-type ions of low charge and low polarizing power; and (c) properties which do not involve the temporary formation of asymmetrical force fields, for example, compressibility, but not plasticity; solubility, but not wettability.

Pauling[3] derived a number of basic rules governing the atomic structures of complex ionic crystals consisting of polyhedra. These rules were very wisely limited to ions having a certain size and charge and a complete octet configuration (noble gas-type ions), because this restriction excludes major polarization effects.

TABLE 1 lists the molar refraction of gaseous ions in cc. according to Bauer and Fajans.[5]

TABLE 1

		He	Li^+	Be^{2+}
		0.521	0.08	.....
O^{2-}	F^-	*Ne*	Na^+	Mg^{2+}
6.95	2.44	1.00	0.47	0.26
S^{2-}	Cl^-	*A*	K^+	Ca^{2+}
22.6	9.06	4.20	2.25	1.40
Te^{2-}	I^-	X_2	Cs^+	Ba^{2+}
40.9	19.21	10.43	6.53	4.73

In TABLE 2, the polarizabilities of some ions (in cubic angstroms) are listed, as

TABLE 2

Li^+	11.7	F^-	25.8
Na^+	20.2	Cl^-	62.8
K^+	42.3	Br^-	81.0
Rb^+	58.1	I^-	109.4
Cs^+	71.3	O^{2-}	30.0
Mg^{2+}	11.4	Cu^+	27.2
Ca^{2+}	34.9	Ag^+	39.4
Sr^{2+}	49.1	Tl^+	90.1
Ba^{2+}	70.6		

taken from the literature by Roberts.[4] Both tables are interesting because they show the connection between polarizability and molar refraction. For the following discussion, however, it is not necessary to use the quantitative data available on this subject. It can be shown that the use of Fajans' rules on the mutual polarization of ions allows us to explain and to predict catalytic phenomena in a fairly satisfactory way.

A Derivation of Some Simple Rules Describing the Change in Chemical Properties of Adsorbed Ions of High Polarizability. Polarizability and atomic or molecular refractivity are proportional and, therefore, it has become customary to express the polarizability of an ion by its refractivity (TABLE 1). The response of an electronic system, however, is not restricted to alternating electrical fields of high frequency, but is just as noticeable in a static electrical field. In atomic dimensions, the ions exert forces upon each other which correspond to electrical fields of several million volts per cm. In the interior of a NaCl crystal, a Na^+ ion finds itself in a symmetrical surrounding and, therefore, in the weak electrical force field between Cl^- ions. This is no longer true for a Na^+ ion in the crystal surface, but the latter is in a strong electrical field with the Cl^- ion on one side and no such anion at the other side to balance it.

For the understanding of surface chemistry, one has to realize that the polarization of a surface ion exposed to one-sided attraction has changed its chemical properties. For explaining catalytic actions, it is not sufficient to express deformation or polarization as the induction of a dipole or multipole, because this terminology has no obvious connection with the chemical properties of the ion. The change in chemical properties can be described only in a qualitative fashion, because we still lack the terminology for describing an electron configuration which corresponds to an intermediate state between two stable quantum configurations. We have, for example, names for Cr^{2+} (chromous ion) and for Cr^{3+} (chromic ion) but not for a unit which on one side has an electron density somewhere between these two configurations. As a result, the "chemical description" of the polarized ion is imperfect; it remains

qualitative in character and must be considered only a first approximation to the actual conditions. Nevertheless, its usefulness in explaining surface chemical properties and in predicting the catalytic behavior of ions should make its presentation worthwhile as a first step in laying the foundation for the science of catalysis.*

Scheme I† shows a Ni^{2+} ion adsorbed at the surface of a noble metal, for example, platinum. A metal represents a highly polarizable medium because of the extreme mobility of its "free electrons." It can easily adjust its own force fields to suit the requirements of its environment. The approach of the Ni^{2+} ion leads to the formation of a "metallic bond" between the Ni^{2+} and the platinum. The nickel ion is polarized to such an extent that it can now be described as consisting partly of atomic nickel and partly of Ni^{4+}. That portion of the nickel ion which is extended toward the solution assumes properties which approach those of nickel in a higher state of valency. As mentioned before, such a description is qualitative in character and one should not take it too literally, but it gives us the right direction in which the properties of a polarizable cation are changed when it is adsorbed at the surface of a noble metal. This change may also be described as an "inner disproportionation."

Rule I. Cations of high polarizability, adsorbed at the surface of a metal, extend metallic bonds towards the metal surface and, as a result of this deformation, their chemical behavior towards the solution resembles that of the same element in a higher state of oxidation.

This description of the polarized cation, in chemical terminology, makes it possible for us to understand and even to predict catalytic phenomena involving noble metals as catalysts.

The development of the latent photographic image is one of the best known examples of a catalytic reaction covered by Rule I. The developing bath contains a reducing agent which is not sufficiently strong to reduce the undeformed silver ions but which can react with those that are deformed by their contact with a noble metal. The catalyst, the silver speck, is the result of photolysis of the silver bromide. The asymmetrical force field induced by the sensitivity speck changes the random distribution of the silver atoms into an agglommeration near the surface. As soon as the number of atoms is sufficient to impart metallic properties to this agglommeration, it becomes a catalyst. Silver ions in contact with this metal speck are polarized in the fashion described above and are then able to accept electrons from the developer solution. The silver ions either may come from the bromide grain (chemical development) or they may be added with the developing solution (physical development). This lowering of the activation energy of the reduction of heavy metal ions by deforming them at a metal-liquid or metal-gas interface is rather widely used.

An ammoniacal solution of a cupric compound does not react with formaldehyde except in the presence of a noble metal, such as Ag, Au, or Pt, where metallic copper may be deposited. Vomer[9] has taken advantage of the catalytic activity of metallic silver on this reaction and has developed a method for depositing copper films on glass. For this purpose, the glass is first coated with a very thin layer of metallic silver and then brought into contact with the ammoniacal copper-tartrate-formaldehyde solution. The silver layer can be so thin that it is invisible, and still it causes the Cu^{2+} ions to be reduced to the metal. According to Rule I, we may describe this catalytic reaction as follows: The metallic film of silver adsorbs Cu^{2+} ions and deforms them in a fashion described by scheme I. A metallic bond is formed between the Ag metal and Cu^{2+} ions by increasing the electron density between the metallic phase and the Cu^{2+} nucleus. This, in turn, causes the cupric ion to resemble a Cu^{3+} or a Cu^{4+} ion towards the solution. It is not surprising that such a deformed cupric ion now attracts electrons from the reducing agent and changes into a neutral atom.

* Work on this subject is being sponsored by the Material Branch of the Office of Naval Research.

† Lantern slides were shown at this point.

This atom then polarizes another Cu^{2+} ion in the same fashion until the reduction of the cupric salt is completed or the reducing agent is oxidized.

The same explanation applies to a method of precipitating metallic nickel on glass which has been developed by Pragst.[10] An ammoniacal solution of a nickel salt containing NaOH does not readily react with hydrazine compounds except in the presence of noble metals which catalyze the reduction of Ni^{2+} to the metal or the oxidation of hydrazine to nitrogen by Ni^{2+} ions.

The surface of a solid containing cations of low polarizability is formed primarily by anions. The higher polarizability of the anions allows them to adjust their electron configurations better and, with them, to decrease their force fields extending into space. This deformation lowers the surface free energy of the solid. Scheme II* represents the adsorption of a silver ion on the surface of alumina, *i.e.*, a surface which consists primarily of anions. The deformation of the Ag^+ ion is such that the electrons are repelled by the carrier so that the Ag^+ ion behaves like an Ag^{2+} ion in the direction towards the carrier. Its asymmetrical electron distribution causes it to assume chemical properties resembling atomic silver in the direction of the solution.

Rule II. Carriers consisting of anions having a low or a medium polarizability and of highly charged cations deform adsorbed cations of the non-noble gas-type in a manner which causes their chemical behavior to be shifted toward that of the same element in a lower state of oxidation ($Ag^+ \rightarrow Ag^\circ$).

This description of the polarized cation will be used to explain some catalytic reactions.

The deposition of a silver mirror on glass is a typical catalytic reaction covered by Rule II. Gardner and Case[11] write: "In commercial silvering many manipulators follow the cleansing of the glass surface with nitric acid by a vigorous swabbing with a saturated solution of stannous chloride ($SnCl_2$) which is carefully rinsed off with warm water. This is regarded as an essential feature in many of the 'secret processes' used in the trade." The adsorbed Sn^{2+} ion which is deformed under the influence of the OH^- or O^{2-} ions of the glass surface can be described as an Sn^{4+} ion in its behavior toward the glass and as metallic Sn in its reactions toward the silvering solution. There are many different processes for depositing silver on glass, but all of them have one thing in common: a mixture of a silver compound with a reducing agent which does not react with noticeable speed but requires a catalyst. The presence of deformed Sn^{2+} ions at such a surface catalyzes the Ag deposition and provides good adhesion according to the scheme:

glass surface $-$ ($Sn^{4+} - Sn^\circ$) $-$ metallic silver.

Between the polarized Sn^{2+} ion and the glass, the strong Coulomb forces characteristic for $O^{2-} - Sn^{4+}$ ions are in effect. Between the polarized Sn^{2+} ion and the metallic silver, one can expect the formation of metallic bonds.

The reduction of Ag^+ ions is catalyzed by the glass alone because the Ag^+ ions can undergo a similar deformation as described for the Sn^{2+} ions. This can be proved by the "metallic" properties of Ag^+ ions adsorbed at a carrier such as glass or Al_2O_3.

The decomposition of hydrogen peroxide is not affected by glass powder or by alumina. The presence of Ag^+ ion in aqueous solution is equally ineffective on the rate of this reaction. This reaction can be strongly accelerated by noble metals such as silver, probably because the surface of a metal, with its high electron density, increases the probability of the electron transfer which leads to O_2 and H_2O according to

$$2H_2O_2 = 2H_2O + O_2.$$

We have found[12] that by adsorbing silver ions at glass or at alumina one can imitate the surface of a metal. The simultaneous presence of Ag^+ ions and glass powder or alumina catalyzes this reaction. The same

* Also illustrated by a lantern slide.

is true for Hg^{2+} ions which are ineffective by themselves but turn into positive catalysts when adsorbed at alumina.

The fact that a carrier influences the chemical properties of an adsorbed ion or molecule is so well established that a chemist takes for granted that the chemical behavior of an adsorbed substance depends on the nature of the carrier. Newton's principle of action and reaction being equal allows us to expect that the behavior of the carrier should also be affected by the nature of adsorbed ions.

Due to their free electrons, noble metals show a number of characteristic catalytic activities such as enhancing the oxidation of hydrogen gas or the decomposition of H_2O_2 molecules in aqueous solutions. The adsorption of ions at the surface of a metal can be described as the result of its producing image forces in the highly polarizable metal surface. The contact of a metal surface with the cube face of a NaCl crystal, for example, produces in the metal a checkered pattern of electrical fields characterized by higher and lower electron densities. The Na^+ ions attract, and the Cl^- ions repel, electrons. As a result, the metal surface no longer permits electrons to flow "freely." In the surface of the polarized metal, the flow of electrons now requires an activation energy, because they have to move through areas with higher electron density from which they are repelled and through areas of lower electron density which have a tendency to retain them.

Rule III. A metal in contact with crystal surfaces or with adsorbed dipole molecules which produce positive and negative force fields has its electronic conductivity changed from that of a true conductor toward that of a semiconductor. The electrical resistivity increases in magnitude and its temperature coefficient decreases. For very thin metal films, the temperature coefficient of the surface resistivity may become negative.

This phenomenon has been observed for thin films of metals evaporated *in vacuo* onto a degassed glass surface. Appleyard[13] was the first to report this unusual behavior of alkali metals and emphasized that this phenomenon cannot be explained by any simple theory of metallic conduction: "It is indeed clear, that no form of mere geometrical disintegration of the film such as spongyness or porosity can give a negative coefficient (of electrical resistivity) to a film if the bulk metal has the normal positive temperature coefficient." The same phenomenon has been found for thin platinum films on glass by Vodar and Mostowitch.[14]

Von Itterbeek and de Greve[15] recently called attention to the negative temperature coefficient for electrical resistance of nickel films having a thickness below 40 mμ. The same authors found that the electrical conductivity of thin nickel films may deviate from Ohm's law and increase with the applied voltage.

For metal films which are sufficiently thin, the normal electrical properties of the bulk metal are decreased at the expense of the semiconducting properties of the surface films. The surface film of a metal shows all the characteristics of a crystal with "available" but not "completely free" conduction electrons: negative temperature coefficient for electrical resistances and deviation from Ohms Law.

This polarization of a finely subdivided metal, either as film or powder, offers unusual possibilities for synthesizing semiconductors. Recent work of Hausner[16] seems to be a step in this direction from a commercial point of view.

The magnitude of the force field of the ionic substance in contact with the metal is very important for this purpose. Compounds such as rubidium iodide cannot be very effective in this respect because the high polarizabilities of the large Rb^+ and I^- ions result in weak force fields (low surface tension). MgO or BeO, in contact with a metal, should be much better suited

for producing semiconductors. The difference in the polarizabilities of a cation and an anion decides which ion is the most likely to be adsorbed at a metal surface. The greater the polarizability of an ion, the better it is suited to bridge the gap between two incompatible media, *i.e.*, to lower the interfacial energy of systems such as metal-gas or metal-aqueous solution. This phenomenon is very important for reactions which are catalyzed by a noble metal, because it shows how the activity of a metal can be poisoned as well as enhanced. Bringing cations of low polarizability (noble gas-type ions) onto the surface of a noble metal accentuates its "metallic properties" because their positive charge raises the electron density in the metal surface. Cations with a complete octet shell have long been used to produce "super metallic" properties in metals. Electronic devices take advantage of the fact that the oxide-coated cathode has a much greater electron emissivity than a pure metal at the same temperature because the polar oxide induces spots of high electronic densities in the polarizable metal as described by Weyl and Forland.[17] The same applies to the catalytic properties of noble metals. As early as 1844, Dobereiner[18] improved one of the first catalysts ever investigated, namely, the platinum sponge, by bringing alkali onto its surface. In the same fashion, Bredig[19] increased the catalytic activity of this metal in respect to the decomposition of hydrogen peroxide by the addition of alkali.

Rule IV. The catalytic activity of a metal surface can be accentuated by bringing cations with medium or low polarizability onto its surface. Their positive force fields increase the electronic density in the metal surface and cause the surface to assume "super metallic" properties.

For this purpose, cations of low to medium polarizability are best suited, because cations of high polarizability change the character of the metal surface completely. A metal surface covered with Pb^{2+} ions or with Hg^{2+} ions has lost its catalytic properties because these cations are so strongly deformed by the metal that the system Metal-Pb^{2+} can be adequately described by the scheme Metal $-$ ($Pb^{\circ} - Pb^{4+}$). As a result, the metallic character disappears and strong ionic forces are extended into the solution or into the gas phase. Chemical reactions which are catalyzed by a surface of high electron density are not likely to occur at a noble metal covered with Hg^{2+} or Pb^{2+} ions. Cations of high polarizability act as catalytic poisons. The same effect can be produced by compounds containing large and, therefore, highly polarizable anions such as S^{2-}, Se^{2-}, P^{3-}, N^{3-}, *etc.*

Rule V. Those catalytic activities of noble metals which are based on the high electron density and electron mobility characteristic for their surfaces are poisoned by cations or anions of high polarizability (Pb^{2+}, Hg^{2+}, S^{2-}, P^{3-}, N^{3-}).

The greater the deformability of ions, the better they can bridge the gap which exists between a metal and a gas or a metal and a solution. These ions lower the free energy of the interface, and for this reason they are preferentially adsorbed and are effective even in very low concentrations.

Just as Pb^{2+} ions can be made to assume the character of Pb^{4+} ions towards the solution by adsorbing them at a metal surface, so also can one polarize them in the opposite direction so that they will assume "metallic properties" towards the solution. This is the case for the surface of a PbF_2 crystal, where the Pb^{2+} ions are deformed by the F^- ions.

The polarizabilities of Pb^{2+} and Br^- ions are both rather high, so that one cannot expect the surface of a $PbBr_2$ crystal to differ greatly from that of the bulk. Going from lead bromide or chloride to the fluoride, one can expect that the surfaces are formed predominantly from Pb^{2+} ions. The surface free energy of a PbF_2 crystal is lowest if the most polarizable ion, in this case no doubt the Pb^{2+} ion, forms the extreme

outer surface layer. The deformed Pb^{2+} ions can be described as extending strong positive force fields towards the fluorine ions (thus behaving like Pb^{4+} ions toward the interior of the crystal). The fluorine ions, on the other hand, repel the two outer electrons of the Pb^{2+} ions in a fashion which can be described as causing these ions to assume the high electron density of the atoms in the outward direction. This reasoning led to the discovery of PbF_2 behaving like a noble metal and catalyzing the decomposition of hydrogen peroxide.[20]

Frequently, the conditions described in these rules may not apply to a reaction from its very beginning, but develop as the reaction proceeds. A typical autocatalytic reaction, *i.e.*, a reaction where one of the reaction products assumes the role of the catalyst, is the reduction of cupric oxide by hydrogen. This reaction will be used to demonstrate the mechanism of autocatalysis and to explain why the presence of elemental copper decreases the activation energy of the reduction of its oxide.

Pease and Taylor[21] found that the induction period of this reaction can be shortened by adding metallic copper to the oxide. The reaction

$$Cu^{2+}O^{2-} + H_2 = Cu + H_2{}^{+}O^{2-}$$

can be described as the Cu^{2+} ions attracting two electrons from a chemisorbed hydrogen molecule, thus forming two protons and a copper atom. In Rule I, the combination Metal-Polarizable Cation has been described as $Me - (Cu^{\circ}Cu^{4+})$. The cupric ion, in contact with metallic copper, is deformed in a fashion which causes it to extend metallic bonds to the metal atom and, as a result, to emanate a stronger positive force field into the gas phase. Consequently, the couple copper-cupric oxide has a greater affinity for the electrons of the hydrogen and can be more readily reduced than the pure cupric oxide alone. This concept tells us why this reaction is autocatalytic and, furthermore, allows one to predict that intimate mixtures of CuO with metallic Au, Ag, or Pt will be more readily reduced by hydrogen than the pure oxide.

Combinations of this type are used for oxidizing carbon monoxide at low temperatures. Silver oxide is readily reduced by carbon monoxide and forms metallic silver and carbon dioxide. The oxides of Mn, Cu, Co, and Ni, when applied in their pure states, do not react with carbon monoxide at room temperature with sufficient speed, but they will do so when mixed with Ag_2O. Very probably, this mixed catalyst (Hopcalite) owes its activity to the formation of atomic silver, which aggregates to form tiny silver specks analogous to the formation of the latent image in the photographic process. A speck consisting of ten or twenty Ag atoms polarizes the cations of the other metal oxides as described above. This catalyst resembles the photographic emulsion, which becomes developable by a weak reducing agent once a silver speck has formed.

Disturbance of the Surface and its Depth Action. It is not possible in this paper to give a complete description of the influence of the polarizability of ions on the surface chemistry of solids. In recent publications, the author described in more detail its effect on the hygroscopicity[22] and the wettability[23] of solids, as well as on the adhesion forces between different solids.[24]

There is one important feature, however, which must be mentioned briefly, because it seems that this particular manifestation of the polarizability of the lattice constituents is not yet generally understood.

The fact that a surface of a solid differs from the bulk is obvious and is well recognized. If one assumes a different surface structure, however, one must also assume a finite distance between this structure and that of the bulk. Bulk structure and surface structure cannot be treated as independent of each other and, consequently, a depth action results which depends upon the polarizability of the constituents.

This depth action might be negligible in substances of low polarizability (NaCl),

but it must be considerable in metals. W. Braunbeck[25] examined the conductivity of a platinum foil 2 microns thick in different gases. The abrupt transition from metal to vacuum creates a disturbance of the surface which must exert a considerable depth action, *i.e.*, it causes the atomic structure of the metal to deviate from its ideal or bulk structure over a region which is many atoms thick. By bringing gases onto the metal surface, the abruptness of the transition, metal-vacuum, is decreased by the adsorbed gas molecules, and the surface film approaches a more orderly arrangement of the atoms, as can be seen from the electric conductivity. Helium, a gas of low polarizability, was found to increase the conductivity of these platinum foils 0.1 per cent over that *in vacuo*. The more polarizable argon and oxygen atoms were found to be three and five times respectively as effective as helium.

The depth action which a disturbance creates in highly polarizable metals can be judged from the fact that it is possible to harden metallic lead by a minor addition of the still softer sodium or calcium metal. Foreign atoms of different size, charge, or electron configuration produce disturbances in the metal which must have considerable depth action in order to be effective in these low concentrations. For chemists interested in catalysis, there are two manifestations of the depth action in metals which are of utmost importance.

The first phenomenon deals with the metallic properties of a noble metal as a function of the particle size. Good electronic conductivity of the surface of a metal requires a certain state of order. In order to have such a surface, it has to be backed up by a deep layer of the metal. This means that the catalytic properties of a noble metal can decrease if its subdivision is carried too far. It has been found, for example, that the catalytic action of a certain quantity of metallic gold first increases with increased surface development, but passes through an optimum in order to decline with further subdivision.

Some data of Rusznyak[26] will be given as an example. The decomposition of 50 per cent of the hydrogen peroxide added to colloidal gold required 14 minutes for a sol having a particle size of 42 mμ. Decreasing the particle size to 31 mμ more than doubled the time, and particles of 29 mμ required 32 minutes. The finer particles are less efficient because their surface structure is less orderly and less conducive to electron transfer than that of a large gold particle. It is well established that a silver speck must have a certain size in order to become a catalyst and to make the silver bromide grain developable.

The strong association between the electronic properties of finely divided metals and the surrounding media is not limited to conductivity (Braunbeck[25]) but manifests itself in the light absorption as well. Nearly one hundred years ago, in 1857, Faraday[27] published his classical studies on the relations of gold and other metals to light and described that the color of finely subdivided metal depends on the nature of the environment: "The effect strengthens in my mind the thought that in the ruby fluids the deposited particles are frequently associates of water and gold. It is a striking case of the joint effect of the media and the gold in this action on the rays of light and the most striking amongst those where the medium may be changed to and fro."

The second manifestation of the depth action is related to the fact that the properties of a metal surface can be changed by backing it up by various carrier substances. The catalytic activity of cobalt or of nickel films can be improved by bringing these metals onto the surface of certain carriers, such as ThO_2. Just as the electron emissivity and the catalytic activity of a metal surface can be improved by bringing certain cations onto the surface (Rule IV), its catalytic properties can be modified by backing the film up with different oxides. One of the most spectacular effects revealing this depth action is an observation made by Schwab[28] and his collaborators.

They found that copper or nickel precipitated onto the surface of dextro-rotatory quartz crystals dehydrates laevo-rotatory butyl alcohol faster than its optical antipode. Normally, these metals, being optically inactive themselves, must catalyze both reactions in precisely the same fashion. The optically active quartz surface seems to impress its force field upon the metal film, however, and the resulting image forces cause a distortion within the metal film which has sufficient depth action to become noticeable on its other side in the form of a different geometry and different lattice distances.

Conclusions. Our knowledge of catalytic phenomena is increasing very rapidly. There is no doubt that crystal chemistry is the natural basis for the understanding of heterogeneous catalysis involving crystal surfaces. Up to the present time, crystal chemistry has been restricted to the treatment of crystals as arrays of ions which extend infinitely in space. Some of the most interesting physical properties (strength, low temperature, electronic conductivity, fluorescence, *etc.*) are governed by the deviations from ideal patterns or by the defects occurring in the crystal. The chemical properties are functions of the surface structure which, too, must be treated as a defective crystal. The parameters used for describing the bulk of the crystal cannot be applied to its surface.

In order to understand better certain color phenomena concerning natural minerals and synthetic pigments, it became necessary to extend crystal chemistry so as to include defective structures. This could be done on the basis which was laid by Goldschmidt, but the emphasis had to be shifted towards ionic deformation. Goldschmidt, in his work on the properties of crystals as related to their chemical composition, treats the size, charge, and polarizability of the ions on a par. This treatment is justified as long as it refers to certain bulk properties. The so-called structure sensitive physical properties and the surface chemistry, however, are most strongly influenced by the polarizability of the ions. In perfect crystals, the size of the ions is a very important property, because it determines their Coulomb forces. On the basis of Fajans' rules, Weyl and Forland extended crystal chemistry to include defective structures and explained some of the unusual color phenomena characteristic for minerals.*

The same ideas have now been used for explaining the chemical behavior of ions adsorbed at various carriers. As far as the qualitative aspects are concerned, this treatment is very simple. There are major difficulties concerning its quantitative treatment, however, and it will probably take a long time to bring this approach to a more quantitative basis.

References

1. VERWEY, E. J. W. 1946. Rec. Trav. Chim. Pays Bas. **65:** 521.
2. STRANSKI, I. N. & K. MOLIERE. 1948. Z. Physik **124:** 421, 429; 1950. *Ibid.* **127:** 168.
3. FAJANS, K. 1941. J. Chem. Physics **9:** 378.
4. ROBERTS, S. 1949. The Physical Review **76:** 1215; 1950. *Ibid.* **77:** 258.
5. FAJANS, K. 1931. Chemical Forces and Optical Properties. McGraw-Hill. New York. References to the later publications of K. Fajans and his associates can be found in Chem. and Eng. News **27:** 900 (1948).
6. GOLDSCHMIDT, V. M. 1926. Geochemical Distribution Laws. VII & VIII. Oslo.
7. FRANKENBURG, W. C., V. I. KOMAREWSKY, & E. K. RIDEAL. 1948. Advances in Catalysis and Related Subjects. I: VII. Academic Press. New York.
8. PAULING, L. 1929. Am. Chem. Soc. **51:** 1010.
9. VOLMER, M. 1924. Ger. Pat. 398108 Kl 32 b. See also E. C. Marboe and W. A. Weyl. The Glass Industry **26:** 119 (1945).
10. PRAGST, W. 1942. Ger. Pat. 717547 Kl 32 b.
11. GARDNER, I. C. & F. A. CASE. 1931. The making of mirrors by the deposition of metal on glass. Circular of the Bureau of Standards N. 389 Washington.
12. ENRIGHT, D. P., E. C. MARBOE, & W. A. WEYL. 1948. Influence of the carrier on the catalytic activity of adsorbed heavy metal ions. Techn. Report No. 1, Contract No. N6 onr 269 Task Order 8, N. R. 032–265.
13. APPLEYARD, E. T. S. 1937. Proceed. Phys. Soc. London **49:** 118. See footnote on page 126.
14. VODAR, B. & M. MOSTOWITCH. 1948. Compt. Rend. **226:** 167.

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15. VAN ITTERBEEK, A. & L. DE GREVE. 1946. Nature **158:** 100.
16. HAUSNER, H. H. 1947. J. Am. Ceramic Soc. **30:** 290.
17. WEYL, W. A. & T. FORLAND. 1949. Crystal chemistry and optical properties of defect structures. Techn. Report No. 2, Contract No. N6 onr 269 Task Order 11 N. R. 032-265.
18. DOEBEREINER, J. W. 1823. Gilb. Ann. **74:** 269; 1824. *Ibid.* **76:** 102.
19. BREDIG, G. 1899. Ztschr. Phys. Chem. **31:** 201.
20. MARSHALL, P., D. P. ENRIGHT, & W. A. WEYL. Unpublished.
21. PEASE, R. N. & H. S. TAYLOR. 1921. J. Am. Chem. Soc. **43:** 2179.
22. ROMAN, M. K., E. C. MARBOE, & W. A. WEYL. 1948. Trans. Soc. Glass Techn. **32:** 260.
23. SONDERS, L. R., D. P. ENRIGHT, & W. A. WEYL. 1950. J. App. Physics **21:** 338.
24. MARBOE, E. C., W. D. SMILEY, & W. A. WEYL. 1948. Trans. Soc. Glass Techn. **32:** 264.
25. BRAUNBECK, W. 1948. Z. Naturforschung **3a:** 216.
26. RUSZNYAK, S. 1913. Z. Phys. Chem. **86:** 681.
27. FARADAY, M. 1857. Phil. Mag. **14:** 401, 512.
28. SCHWAB, G. M., F. ROST, & L. RUDOLPH. 1934. Kolloid Ztschr. **68:** 157.

SECTION OF MATHEMATICS AND ENGINEERING

ELECTROMAGNETIC COMPUTING DEVICES*

By DONALD HERR†

This paper deals with the generalized transfer-functions available through the use of electromagnetic computing devices which consist of two concentric, right circular cylindrical, magnetic structures, each containing generalized conductor distributions, numbers of poles, skewnesses, and physical dimensions. Such devices, when used as voltage- (or other) transfer devices, in terms of the angle of relative rotation between the two magnetic structures, find great use in high-precision, continuous computing applications in analogue computers and control systems. For example, such electromagnetic induction devices may be used to provide output voltages which are proportional to the sine or cosine of the angle of relative rotation, in which case they are called resolvers; for developing output voltages which are linearly proportional to the angle of rotation itself, in which case they are called linear potentiometers; or, for the generation of other useful, arbitrary output-voltage functions of the angle of rotation.

One such specific, but by no means limiting, example is the electromagnetic induction resolver. Prior to World War II, the computing operations of vector resolution and composition, the solution of plane and spherical triangles, and the transmission and reception of position data between remote points were performed by mechanical resolvers almost entirely. These mechanical devices were essentially "Scotch-yoke" mechanisms and were large in volume, heavy, precision-machined, expensive, and seriously subject to the effects of mechanical shock and vibration. Since that time various embodiments of electromagnetic induction resolvers have appeared. One of the specific results of the research, development, design, and manufacturing program which has made this paper possible is the creation of a new high-precision, universal-frequency resolver weighing only approximately one pound, having a volume less than 18 cubic inches and a functional error less than ± 0.05 of 1 per cent over 360 degrees of rotation. In addition, it possesses greatly reduced noise content over a wide range of excitation voltage and, when used with its appropriate compensating amplifier, it operates on any carrier frequency, from 50 cycles per second to greater than 2 kilocycles per second.

Essentially, the fundamental problem has to do with the control of the amplitudes of the space harmonics of the Fourier Series in describing, in most general terms, the arbitrary space (a.c.) flux-density function which it is desired to generate in the magnetic (air) gap between stator and

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, May 19, 1950. This was the inaugural meeting of the Section. This is a condensation and does not contain all material presented or available.

† Reeves Instrument Corporation, New York, N. Y.

rotor structures by the generating winding. The generating winding may be situated on either the stator or rotor structure. For purposes of dissertation, it is assumed to be on the stator structure. The problem also has to do with control of the amplitudes of the space harmonics of the angle of relative rotation between stator and rotor structures, which are induced in the output, or rotor winding. The fundamental aspect of the problem of voltage-transfer-function devices is to control thereby the amplitudes of the space harmonics of the output voltage as a function of angle to the prescribed Fourier coefficients of the arbitrary function desired. This control is accomplished ideally through the proper choice of the number of stator poles, in the slots between which the proper conductor distribution of the generating stator windings are located, by appropriate skewness of the stator slots, by a proper choice of the number of rotor poles, by appropriate conductor distribution in the rotor slots between these poles, and by choice of the proper skewness of the rotor poles.

For the type of symmetry of interest and for a fundamental space period of 360 degrees of rotation (one revolution between stator and rotor), the numbers of the stator and rotor poles are restricted to those which are an integral multiple of four or an integral multiple of four plus two. On the same basis, it is ideally possible to eliminate all even-order space harmonics from consideration, *ab initio*. Hence, it is necessary to deal only with the amplitudes of all odd-order space harmonics.

In the case of a resolver, the basic requirement is that the rotor output voltage consist only of the fundamental space-harmonic of the angle of rotation between stator and rotor, the fundamental period being one full revolution. Hence, the conditions are that all of the possible odd-order harmonics greater than the first to be eliminated ideally by the foregoing means. Other requirements exist, of course, such as the engineering requirements that the fundamental space-harmonic output voltage which does appear be proportional to and approximately equal in maximum value to the generating winding excitation, and that the ratio of reactance to resistance of the generating stator winding be close to the physical maximum.

It is shown for each of the two possible numbers of poles (slots), on stator and rotor, that there exist two different possible conductor distributions. Hence, four basic pole and conductor distributions exist. These are denoted as types A, B, C, and D, types C and D referring to the two possible arrays for magnetic structures with number of poles (slots) equal to an integral multiple of four.

For a double winding stator, namely, two distributed stator windings located on the same stator structure but with axis 90 mechanical degrees apart in space, if one of them is a type A, then the other is a type B; if one of them is a type C, then the other is a type C; and if one of them is type D, then the other is type D.

From the standpoint of complete suppression of unwanted space harmonics, the most superior suppression patterns are obtainable with types A and D stator windings. Type B, the necessary mate of type A, is considerably inferior to type A, whereas type C is inferior in its space-harmonic suppression pattern to all other types. Furthermore, even accepting the inferior type B winding, the use of a type A-type B pair means two different stator winding problems and two different stator winding impedances, as well as two different space-harmonic rejection patterns.

On the rotor, the same four types of pole-and-conductor arrays are possible. Here, however, because of one more degree of freedom, the types A and B are equal in their space-harmonic rejection patterns, as are the types C and D.

For both stator and rotor types A and D, the only space-harmonics which it is not possible to reject simultaneously and completely are those which are equal in order to an integral multiple of the number of poles (slots) plus and minus one.

While the generating structure (here denoted as the stator), as well as the rotor structure, may have its poles (slots) skewed for various beneficial results, it is readily demonstrated that skewing of the rotor slots alone one slot pitch will result in the further suppression of just those pairs of space-harmonics which it is not possible to eliminate completely by rotor pole-and-conductor array.

Ideally then, the problem consists preferentially of using type D pole-and-conductor arrays on both stator and rotor and skewing the rotor poles one slot pitch. For example, in a 20-pole type D stator, with the exception of the wanted first space-harmonic or space fundamental, the only other space harmonics which are generated in the air gap are the unwanted ones, namely, 19th and 21st, 39th and 41st, 59th and 61st, 79th and 81st, *etc.* Each of these appears with an amplitude with respect to the fundamental equal to the reciprocal of the order of the harmonic. As far as the type D rotor is concerned, with the exception of the desired first space-harmonic or space fundamental, in a 12-pole (slot) rotor it is possible to suppress entirely all other unwanted space harmonics except the 11th and 13th, 23rd and 25th, 35th and 37th, 47th and 49th, 59th and 61st, *etc.*, by means of the appropriate conductor distribution. Without regard to skewing, the amplitudes of the remaining rotor harmonics are proportional to, among other factors, the reciprocal of the order of the harmonic, with respect to the fundamental amplitude. As noted, it is possible, by use of rotor skewness, to suppress the amplitudes of these remaining unwanted space-harmonics of the rotor still further. By use of unity skewness, noted above, the ratio of these unwanted space harmonics with respect to the fundamental, in amplitude, becomes equal to the reciprocal of the square of the order of the harmonic.

Hence, in such a combination, the first unwanted harmonic which the stator generates and the rotor has induced in it consists of the pair, the 59th and 61st orders, and these bear in amplitude a ratio to the fundamental of approximately the reciprocal of 60 cubed, in the ideal situation.

Other considerations enter into the design of high-precision, universal-frequency computer components of this type. Chiefly, they involve the use of a high permeability, low core-loss, high saturation point, homogeneous, annealed magnetic material in both the stator and rotor structures; the proper choice of stator and rotor slot shapes and slot openings; the cleanness of the magnetic punchings and their care; the minimization of leakage flux; the proper choice of air gap width and the reduction of end and slot leakage flux; extreme concentricity of stator and rotor structures (mechanically, magnetically, and electrically); optimum ratio of stator and rotor lamination stack lengths to rotor radius and outside stator radius; maximum reactance-to-resistance ratio; and finally, the manner of winding the conductors into the slots.

The foregoing techniques can insure minimum noise voltages being generated in the electromagnetic devices, which are considerations apart from the space-harmonic distribution problem treated above.

With the electromagnetic computing device itself of optimum design, the compensation of the electrical phase-angle of the rotor output voltage, with respect to the stator input voltage, and the precise adjustment of the ratio of rotor output maximum amplitude to the amplitude of the stator input or excitation voltage require the use of a high-gain, feedback, booster-amplifier compensator. The feedback path may consist of either compensating networks or compensating networks plus an internally wound feedback stator winding. In the same unit, temperature and amplitude linearity compensation may be effected.

By the foregoing means, the residual unwanted time-quadrature and time-harmonic noise components are greatly reduced to less than a total of one part in a thousand of the maximum rotor output voltage, over 360 degrees relative

stator-rotor rotation, over an input range which increases from 60 volts maximum at 60 cycles per second carrier frequency to greater than 300 volts maximum at 400 and 1000 cycles per second carrier frequency, in the same device. Additional practical factors involve the provisions for least weight and volume of the computing device and its booster-amplifier, extreme shock and vibration proofness, corrosion resistance, and minimum rotor inertia and friction.

Entirely subject to similar considerations are high-precision universal-frequency linear induction potentiometers and other voltage- (or other) function generators. Similarly, these units are provided with double-ended shafting as well as single-ended shafting and any combination of one or two stator windings and one or two rotor windings.

Based upon the results of the fundamental research, development, and design program outlined herein, the Reeves Instrument Corporation is now in full production and test of machine-wound high-precision, universal-frequency a.c. induction computing components of the type described to fulfill its own stringent needs and the needs of others in large quantities, with assured delivery and reasonable cost.

In conclusion, I would like to acknowledge the unstinting, faithful, and most competent work of my two chief assistants, Mr. John D. Loy, Master Instrument Machinist, and Mr. Norman R. Blumenstock, Electronics Engineer, in this program. Very special mention is made of Mr. Harry D. Belock, Executive Vice President and General Manager of the Reeves Instrument Corporation, whose insistence upon there being made available such a high quality resolver motivated much of this work. I express my deepest thanks to Miss Sophie Regan for her painstaking secretarial help and encouragement.

*SECTION OF OCEANOGRAPHY AND METEOROLOGY**

INDUCED PRECIPITATION AND EXPERIMENTAL METEOROLOGY†

By VINCENT J. SCHAEFER‡

In considering the general aspects of "induced precipitation," there are a few basic scientific facts which must be recognized at the outset if we are to make substantial progress in evaluating the possibilities and limitations of cloud modifications produced by artificial means. There are three factors in the physical structure of the atmosphere which seem to be of primary importance in this respect. These are (1) supercooled clouds, (2) ice nuclei, and (3) the synoptic situation.

* On May 22, 1950, the Section of Anthropology held a meeting at which Marian W. Smith read a paper entitled "Testing Anthropological Field Methods." No abstract of this paper has been received.

† On May 23, 1950, the Section of Oceanography and Meteorology held a symposium, presided over by A. F. Merewether of Pan-American Airlines, on "Induced Precipitation." The first paper, of which no abstract has been received, was read by Wallace E. Howell, Harvard University. The present paper was the second of four presented at the symposium.

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Supercooled Clouds. Supercooled clouds are of common occurrence in the natural atmosphere. In fact, it is most likely that all natural clouds supercool whenever their individual droplets cool below 0°C.

A supercooled cloud is inherently unstable. This is due to the difference in the partial vapor pressures of water and ice which reach a maximum at about −14°C. At all temperatures below 0°C, however, an ice crystal will grow at the expense of the nearby supercooled cloud droplets, which evaporate and condense in the solid phase.[1] The shift from water to ice causes an increase in temperature within an affected cloud. This heat of sublimation may range from a few hundredths of a degree in a thin stratus cloud to two or three degrees Centigrade in a large cumulus cloud. The speed of transformation may require ten or fifteen minutes or only a few seconds, depending on the concentration of ice nuclei. Very rapid transforma-

tions occur only when the concentration of ice nuclei is of the order of $1 \times 10^8/M^3$, a condition which has not yet been reported to exist in the natural atmosphere and one which probably never occurs in clouds except at temperatures colder than −39°C.

It is a simple matter, however, to produce such a condition on a large scale by artificial means, using either dry ice or silver iodide.[2,3] It is this fact which encourages us to believe that experimental meteorology has an important future in certain phases of meteorology.

The persistence of supercooling thus seems to depend primarily on the temperature of the supercooled cloud and the presence or lack of a critical concentration of ice nuclei. In the absence of a sufficient concentration of ice nuclei within a cloud, it may supercool to a temperature of −39°C. This is the critical temperature at which spontaneous nucleation occurs.[4] It is now believed that except in very special cases, which are quite unlikely to occur in the natural atmosphere, supercooling will not extend beyond −39°C.

Ice Nuclei. Many observations of natural clouds have indicated the great variability in concentration of ice nuclei in the free atmosphere. It is because of this fact that large vertical thicknesses of supercooled cloud sometimes occur. In fact, it is often observed that large towering cumulus reach the −39°C isotherm with substantial quantities of the cloud still in a supercooled state. The transformation to ice crystals under such conditions is rapid and sometimes quite spectacular, although optical effects due to this transformation are generally absent, since the cloud droplets which freeze probably remain spherical. At other times, the cloud shifts over to snow at a warmer temperature. This variability in the persistence of supercooling is consistent with the physical facts, which are now fairly well understood due to recent work in Europe and this country.[2,5]

The concentration of ice nuclei in the free atmosphere has been the subject of "Project Cirrus" field observations[6] for more than two and a half years. Nearly 6500 observations have been made at the Mt. Washington Observatory as part of this program. Air passing the mountain summit at an altitude of about 6300 feet above sea level is introduced into a cold chamber, a supercooled cloud is formed at a temperature of −15°C to −18°C, and the number of ice crystals which form within the cloud are noted. An analysis of these data shows that the concentration of potential ice nuclei in the air which passes the summit of Mt. Washington varies by a factor of more than a million-fold. These experimental facts are of great importance in a consideration of what might be accomplished by the seeding of unstable clouds by artificial means.

Similar studies of natural ice nuclei now underway in this country and England substantiate the Mt. Washington studies. In addition, they disclose critical temperatures below which there seems to be a sudden increase in nuclei. In New Mexico, this temperature is about −22°C;[7] in England, it is reportedly[8] at about −32°C. This increase is of a much lower magnitude than the critical spontaneous nucleation temperature of −39°C, but it may go from $1 \times 10^2/M^3$ to $1 \times 10^6/M^3$ between −19°C and −23°C. The 10,000-fold increase in potential ice nuclei over a temperature range of 4°C is quite in accord with the characteristic activity of foreign particle nuclei which has been studied by the writer.[2,6] Many natural soils from the region of the Great Plains and the Northwest, when introduced into a cold chamber as a fine dust cloud, are quite useless as ice nuclei at −13°C but 100 percent active at −22°C. The wide range in activity which has been found between various samples of such foreign particles and the randomness in the manner whereby they become airborne particles seems to provide a logical answer to the variability and unpredictability in occurrence of supercooled clouds.

The Synoptic Situation. The general weather situation which accompanies the

formation of clouds may have a profound effect on their subsequent development and possible precipitation. The presence of a strong inversion or a layer of dry air aloft limits the formation of thick cumulus clouds. The lack of sufficient moisture or a favorable lapse rate also imposes restrictive limitations on the development of clouds. Any feature of the synoptic situation which puts a restraint on the convergence of moisture laden air also exerts its effect on the development of precipitation.

When areas of unstability are present in warm, moisture laden air or when a strong flow of such air encounters heated mountain ridges or a dense mass of cold air, conditions are potentially suitable for the development of precipitation. The almost infinite variation in the synoptic situation which occurs in the atmosphere makes it difficult, however, to establish any simplified picture of such situations. Langmuir has pointed out[9] that the formation of storms is an excellent example of divergent phenomena—the development of very complicated systems from simple beginnings. It follows from this concept that the difficulties still experienced in preparing reliable forecasts of certain types of storms, especially those which suddenly burst into full vigor without prior warning, are prime examples of divergent phenomena. In some cases, even with much additional physical information than is now available, it may always be difficult, if not impossible, to predict the occurrence of such storms with any degree of accuracy.

Aside from these complicated and difficult problems, it is a fact that there are many features of clouds and cloud systems which we may hope to understand eventually.

The Economic Aspects of Cloud Modification. Due to the complex features of the atmospheric processes which produce substantial quantities of snow and rain on the ground, it is my considered opinion that it is too early to reach definite conclusions as to what can or cannot be done with respect to squeezing additional amounts of moisture out of clouds.

A very important paper related to this problem was recently presented in Washington by Braham,[10] which, on the basis of a study of some 53 thunderstorms in Florida and Ohio, showed that only about 10 per cent of the moisture flowing into a thunderstorm system reaches the ground as precipitation. This precipitation represented about 20 per cent of the condensed cloud water.

If, by careful artificial seeding techniques, a method could be devised to employ the potential energy of the synoptic situation to increase the amount of precipitation, reduce the torrential rain and violent winds, and, at the same time, prevent the formation of hail and lightning, few persons would dispute the economic value of such an operation. While it is too early to say with certainty that such results can be achieved, it seems very possible that such operations are feasible, if in fact they have not already been obtained in some of our New Mexico experiments.[11]

The formation of extensive earth-shadowing clouds by the development of "false" cirrus is another aspect of experimental meteorology which demands careful study. Langmuir has described[11] such formations in sub-tropical regions which cover thousands of square miles. The writer has made observations in New Mexico[6] concerning the ground temperature with and without such snow streamers and found the temperature 2 mm. below the surface to be 140°F on a cloud-free day and 70°F when thin cirrus streamers drifted from the top of a towering cumulus about 50 miles downwind. In this particular instance, although a large cloud formed, it not only reduced the possibility of the formation of orographic cumulus in the shadowed area but the condensed moisture in the cloud formed no precipitation either from the thin cirrus carried downwind or below the cloud responsible for this formation.

In another series of observations, our "Project Cirrus" group[12] studied cumulus and stratus clouds both off the coast and over the mountain ridges of Puerto Rico.

We found measurable amounts of persistent light rain to come from stratus clouds 300 feet thick and fairly heavy rain to fall from relatively small and warm cumulus clouds. These cloud systems seemed to be very efficient producers of precipitation.

It will be very difficult to establish with certainty the value of a cloud-seeding operation in the Northeast, such as is underway in the local New York City watershed. In general, it seems that whenever substantial cloud systems develop in this region rain occurs. Whether the clouds are efficient precipitators like those of Puerto Rico or have properties like the Florida and Ohio thunderstorms, which seem to deliver only 10 to 20 per cent of their condensed water as precipitation, it is important at this stage to evaluate these potentialities. Such factors as the source of the moisture, the concentration and nature of the condensation and sublimation nuclei, the effect of the mountainous terrain in the reservoir area, and the contact between different types of air masses—these are factors which are important and measurable with respect to the local situation.

There is a chance that proper seeding techniques applied at the optimum time can add an appreciable amount of water to the reservoirs. As in any pioneering venture, chances must be taken. If taking such chances does not involve prohibitive costs and if the experiments are planned in such a manner that a scientific evaluation is possible after a reasonable time period has elapsed, then the money and effort are not wasted. If seeding operations over the New York watersheds can be shown to have no apparent economic value, even this information is valuable to have.

We do not yet know which factor or combination of factors are of primary importance in the development of precipitation. Much progress has been made in this field during the last few years and new developments are taking place rapidly. As experience is gained in the Northeast, the Rockies, the semi-arid regions of the Southwest, in California, Australia, Switzerland, France, Canada, Cuba, South Africa, and the many other parts of the world where techniques in experimental meteorology are being developed, the possibilities and limitations of this new science will gradually become delineated.

Those responsible for the establishment of a working experiment in the New York region are to be congratulated on their persistence in carrying out their plans despite various types of obstacles. The over-all value of this activity, quite regardless of its outcome, will be far and above the value of the water that might be added to the pipes flowing into New York City. It is my personal feeling that the type of experiment under study by New York City is one of the most difficult in experimental meteorology. By contrast, the prevention of local hail storms, lightning, and torrential rains should be easy. Such things are relative, however, and although we can now suggest some of the possibilities and limitations in this field of endeavor, there remain many problems in transposing ideas and theories to practical applications.

As in the development of any idea based on an assemblage of scientific facts, progress is sometimes slow and discouraging; at other times, it is rapid and breath-taking. Experimental meteorology seems destined to enjoy both types of progress. Wild speculation is particularly easy with a subject so close to all of us as the weather.

With the establishment of a stable scientific structure as its basis, the true facts will emerge at a speed consistent with the industry, imagination, enthusiasm, and integrity of the individuals and organizations concerned with its development. I see no reason at the present time why it should not become one of the important developments in meteorology in the years ahead.

References

1. BERGERON, T. 1933. On the physics of clouds and precipitation. Mém. Union géod. géophys. int. Lisbon.
2. SCHAEFER, V. J. 1949. The formation of ice crystals in the laboratory and the atmosphere. Chem. Rev. **44:** 291–320.

3. VONNEGUT, B. 1949. Nucleation of supercooled water clouds by silver iodide smokes. Chem. Rev. **44:** 277–289.
4. SCHAEFER, V. J. 1948. The production of clouds containing supercooled water droplets or ice crystals. Bull. Amer. Meteorol. Soc. **29:** 175–182.
5. FINDEISEN, W. 1938. Colloidal meteorological processes in the formation of atmospheric precipitation. Meteorol. Z. **55:** 121–133.
6. SCHAEFER, V. J. 1949. The occurrence of ice crystal nuclei in the free atmosphere. Proc. of First Nat. Air Pollution Symp.: 26–35. Pasadena.
7. REYNOLDS, S. E. Research and Development Division, New Mexico School of Mines. Private communication.
8. CWILONG, B. M. 1947. Sublimation centers in a Wilson cloud chamber. Proc. Roy. Soc. **190:** 137.
9. LANGMUIR, I. 1943. Science, common sense, and decency. Science **97:** 1–7.
10. BRAHAM, R. R., JR. 1950. Water budget of the thunderstorm. Paper presented before the Washington, D. C. meeting of the American Meteorological Society, May 3.
11. LANGMUIR, I. 1950. Progress in cloud modification by Project CIRRUS. Occasional Report No. 21, Project CIRRUS. 25 pp. General Electric Research Laboratory, Schenectady, New York.
12. SCHAEFER, V. J. 1949. Report on cloud studies in Puerto Rico. Occasional Report No. 12, Project CIRRUS. 16 pp. General Electric Research Laboratory, Schenectady, New York.

THE IMPORTANCE OF ARTIFICIAL NUCLEATION FOR THE PRODUCTION OF PRECIPITATION*

By WILLIAM LEWIS†

One of the most important questions involved in current discussions of the effects of cloud seeding is the problem of the reliability of natural ice-forming processes in the atmosphere. Since the proponents of artificial rainmaking are willing to concede that the meteorological conditions must be "suitable," we must consider carefully just what conditions are indeed suitable for the formation of appreciable precipitation and whether natural processes can be relied upon for the release of moisture under these conditions. The basic question is this: how frequently does it happen that the occurrence of precipitation is prevented by an absence of ice-forming nuclei when conditions are otherwise favorable for the occurrence of rain?

My own opinion on this question is that the basic cause of precipitation is the lifting of moist air, and that when suitable conditions of lifting occur, the necessary ice crystals are formed by natural processes. Any exceptional cases in which the occurrence of precipitation may be prevented by a lack of ice-forming nuclei are so rare or the amounts of moisture involved are so small that their importance is trivial.

First let us consider the type and composition of clouds in those situations in which sufficient lifting occurs for the formation of appreciable rain. The formation of light to moderate precipitation from stratiform clouds requires sufficient horizontal-velocity convergence in the lower layers over a considerable area to produce average vertical velocities of the order of 1,000 feet per hour, affecting a layer of the order of 10,000 feet or more in vertical extent. This process causes the formation of very deep, continuous clouds of the type known as "nimbostratus" or "altostratus." If a lack of nuclei were an important factor in preventing rain in cases of this kind, one would expect to find supercooled liquid clouds of from 5,000 to 10,000 feet or more in vertical extent. Observations show, however, that in all cases investigated cloud systems of this type are composed mainly of ice crystals in the portions above the freezing level. Investigations of the extent and liquid-water content of supercooled clouds of various types have shown that supercooled cloud layers seldom, if ever, exceed 3000 feet in vertical extent.

The other principal type of rain-producing cloud is the "convective" or "cumulonimbus" type. Clouds of this type consist of local updrafts, usually rising to considerable heights and characterized by relatively high vertical velocities. Three types of convective clouds are generally

* On May 23, 1950, the Section of Oceanography and Meteorology held a symposium, presided over by A. F. Merewether of Pan-American Airlines, on "Induced Precipitation." This paper was the third of four presented at the symposium.

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recognized: (1) cumulus humilis, clouds having relatively slight vertical development; (2) cumulus congestus, clouds with greater vertical development but without spreading or softening of the tops; and (3) cumulonimbus, clouds with great vertical development, characterized by spreading or softening of the tops and precipitation or virga from the bases. It is generally agreed that a cumulus congestus cloud becomes cumulonimbus when ice crystals are formed within it.

The amount of rain that may fall from a cumulonimbus cloud depends on the amount and moisture content of the air included in the updraft, the amount of mixing with the environment, and the vertical extent of the updraft. The first factor mentioned, the amount of air carried upward in the rising column, is of greatest importance. In cases where the warm moist layer near the surface is unusually thick or when its thickness tends to increase due to horizontal convergence, the ample supply of moist air produces updrafts which are likely to continue for a considerable time, finally reaching a stable layer and spreading out at the top. If, on the other hand, the warm moist layer is shallow, cloud development is often arrested due to local exhaustion of the moisture supply, and the clouds subside and dissipate by mixing with the surrounding air. Since the amount of rain that may be produced depends upon the amount of air that is lifted, most of the precipitation from cumulus-type clouds should be expected from those in which the updraft continues for a considerable time.

Careful observations of the development of cumulus and cumulonimbus clouds and a study of lapse-time motion pictures indicate that the lifetime of a single cumulus congestus tower is usually only about 10 to 20 minutes. These clouds nearly always either subside and dissipate or become cumulonimbus by the formation of ice crystals within that time. It is significant that, when a cumulus cloud does not subside but continues to rise after it has been visible for about ten minutes, it almost invariably starts to bulge at the top and shows evidence of ice crystals within the next four or five minutes. These remarks are based on my own observations of cumulus cloud development in California, New Mexico, New York, and Virginia, and on a study of photographs and lapse-time motion pictures made by "Project Cirrus."

These observations have led me to propose the hypothesis that the natural process of ice formation in clouds usually requires that the cloud-mass involved must exist at a fairly low temperature for a certain time interval, which is probably a function of the temperature (for example, about ten minutes at $-15°C$ and longer periods at higher temperatures). On the basis of this hypothesis, the principal factor which determines whether a particular cumulus congestus cloud will become cumulonimbus is the duration of the updraft. If the supply of warm moist air gives out after about 10 minutes or less, the cloud will dissipate rapidly by mixing with the environment without the formation of ice crystals, while, if the updraft is sustained for 20 minutes or longer and reaches temperatures below about $-10°C$, the transformation to cumulonimbus is almost certain to occur.

It should be noted that it is just these clouds, consisting of sustained updrafts involving large amounts of moist air, that can be expected to produce appreciable amounts of precipitation. Updrafts which last for a few minutes only do not ordinarily contain sufficient moisture to produce more than traces of rain or very light showers.

Let us consider what would happen if, due to an absence of ice-forming nuclei, a sustained and vigorous updraft should fail to change to ice crystals. Since the cloud could not continue indefinitely upward, it would spread out at the top, forming a sort of anvil, which, however, would be altocumulus in form rather than cirroform. Altocumulus clouds of this type, formed by the spreading of cumulus cloud tops, are sometimes observed but usually only when

the stable layer which limits the updraft occurs at a temperature above freezing or at least above −10°C. I have never personally seen a spreading top on a tall cumulus cloud which was not fibrous in structure, showing the presence of ice crystals.

This hypothesis concerning the time factor involved in the formation of ice crystals is also consistent with observed facts concerning the composition of layer-type clouds of various kinds. The most common type of supercooled layer cloud is the stratocumulus or altocumulus which is formed in the upper portion of a turbulent layer of air. The thickness of this layer of turbulent mixing is usually substantially greater than the thickness of the cloud, since condensation only occurs in the upper, cooler portions of the layer. The individual parcels of fog which make up the cloud are therefore being continuously formed by condensation in the rising currents and dissipating by evaporation in the descending currents. Thus, the life of an individual parcel of condensed fog is only a few minutes, although the cloud layer as a whole may last for several hours. The reality of this process of continuous formation and dissipation is clearly shown in lapse-time motion pictures of broken stratocumulus clouds. According to my hypothesis, the reason ice crystals are not usually found in clouds of this kind is that the life of the individual fog parcels is less than the time required for the formation of ice crystals.

This turbulent mixing process gives rise to layers of limited thickness, usually not exceeding 3000 feet. Very thick cloud layers, on the other hand, are generally produced by the lifting of large masses of air by convergence, as was mentioned earlier. In clouds of this type, the individual parcels remain in a saturated state for long periods, allowing ample time for natural ice-crystal formation.

To sum up the argument thus far, it has been shown that the types of cloud which can produce appreciable amounts of precipitation (that is, those formed by the gradual lifting of extensive air masses and those formed by sustained local updrafts involving large amounts of air) are just the types in which the natural formation of ice crystals is observed to occur with great regularity. These are facts of observation. In addition, a hypothesis has been proposed which is consistent with these observations and also with the fact that ordinary stratocumulus and altocumulus layers and short-lived cumulus congestus clouds are generally composed of supercooled drops.

Since some types of clouds are frequently observed to be supercooled, let us consider how much precipitation could be obtained by converting them to ice. Calculations based on the values of liquid-water concentration measured by the National Advisory Committee for Aeronautics show that a typical supercooled stratocumulus layer would yield almost enough precipitation to reach the ground, while one cloud in 100 would produce as much as .01 inch. Similar calculations for cumulus clouds show that as much as .01 or .02 inch could be expected frequently and, in extreme cases, as much as .10 inch might possibly be obtained. It is extremely doubtful that a cloud capable of producing more than .10 inch would fail to precipitate naturally.

So far I have not discussed the mechanism of ice crystal formation in clouds: what kind of nuclei are required, or whether these act as sublimation nuclei, as freezing nuclei, or as "motes" which promote the freezing of cloud drops. A considerable amount of information concerning the formation of ice crystals in the laboratory under certain conditions is now available as a result of work done by several investigators. I will not discuss these experiments in detail, but would like to point out that the conditions occurring in the clouds have not been reproduced in the laboratory. It is not possible, therefore, on the basis of laboratory experiments, to conclude that

natural air does not contain whatever particles may be necessary to promote the formation of ice crystals in a cloud that has aged for, let us say, 15 minutes at −10°C. Observations of ice crystal formation in a cold box on Mt. Washington indicate that the concentration of ice crystals formed under the conditions of the experiment is sometimes very small, but this does not prove that natural ice crystals would not form in the same air under the conditions prevailing in natural clouds. In my opinion, observations of the composition of natural clouds furnish much more convincing evidence on this point. The fact that many cases of extensive cloud systems more than four or five thousand feet thick and with temperatures below freezing were observed to be composed mainly of ice crystals, without exception, is very significant evidence of the dependability of natural ice-forming processes.

Let us consider for a moment the results of actual field tests of cloud seeding. In the case of layer-type clouds, the fact of the possibility of cloud modification has been very convincingly demonstrated. Equally convincing is the failure of all seeding experiments on layer clouds to produce more than trivial amounts of precipitation.

In the case of cumulus clouds, the evaluation of the results of seeding experiments presents a very difficult problem, due to the impossibility of determining, in any particular case, what the natural course of the weather would have been if the experiment had not been performed. The only way that the effects of seeding cumulus clouds can be honestly evaluated is to make as many experiments as possible and study the effects in all cases, observing at the same time the behavior of unseeded clouds nearby. The Weather Bureau, in cooperation with the Air Forces, has conducted two different series of tests on cumulus clouds. The reports of these tests include tables showing, for each seeding operation, the condition of the cloud at the time of seeding, its subsequent development, and the amount of precipitation. The general result of these tests is to show that the most frequent occurrence following seeding is partial dissipation of the seeded cloud and that, occasionally, small amounts of precipitation occurred, but only when natural precipitation was occurring nearby.

Experiments conducted in Canada gave somewhat more favorable results. Of a total of 33 trials with supercooled cumulus clouds, 36 per cent gave no precipitation, 27 per cent gave only virga, and 36 per cent were followed by precipitation reaching the ground. In 8 out of 12 of the cases of rain reaching the ground, natural rain was also occurring within 25 miles. The other four cases were all of the "selective" type, that is, an effort was made to choose the most favorable cloud in the area. Under these conditions, there is a good reason to suspect that these showers would have occurred without seeding.

In summary, my point of view is that, although cloud modification by nucleation has been demonstrated, significant changes in the amount and distribution of precipitation can not be accomplished by cloud seeding, since the distribution of precipitation is determined primarily by air circulation rather than by nuclei. Whenever circulation patterns exist which involve sufficient lifting for the production of appreciable amounts of precipitation, the natural processes of ice-crystal formation are effective in releasing the available moisture.

The possibility remains, however, that the character of precipitation might in some cases be modified by seeding, and I believe that, if successful practical applications of cloud seeding are to be found, they are likely to lie in the field of hail-prevention rather than rain-production. Any estimate at this time of the probability of success would be only a guess, but it appears to me that experiments along this line would be worthwhile.

LARGE SCALE CONTROL OF WEATHER BY INTRODUCTION OF SUBLIMATION NUCLEI INTO THE ATMOSPHERE*

By WILLARD J. PIERSON, JR.†

During the past few years, a great many statements have been made concerning the possibility of producing rainfall and controlling the weather by the introduction of sublimation nuclei into the atmosphere. In fact, some claims of success have appeared in the press. It is the purpose of this paper to point out that some of the statements which have been made are not founded in fact, that the techniques which have been employed are inadequate, and that, in most of the studies which have been conducted, there has been an overall disregard of the scientific facts upon which the theory of meteorology is based.

In the preceding paper, Mr. Lewis has given some reasons for his belief that the theory upon which the possible control of the weather is based is not well founded. His contention that there is always a sufficient number of nuclei present in the atmosphere to produce all of the rainfall that would naturally occur seems to be well supported. There is a danger that too many nuclei will be released into the atmosphere. Too many nuclei prohibit the production of rainfall by causing the formation of a great many small ice crystals which are too small to fall and grow into large rain drops when melted. Dr. Schaefer has done a fine job in measuring the sublimation nuclei count at Mount Washington. However, many more observations, tabulated according to air mass, elevation, air trajectory, and cloud type, are needed before it will be possible to prove that situations exist in which artificial nucleation is advisable.

One of the claims of success which has been made will now be discussed. In a recent article in "Life," dated February 20, 1950, the claim was made that a cloud about twenty-five miles away from a silver iodide nuclei generator was seeded by the silver iodide and developed into a full scale thunderstorm. In addition, it was also claimed that the silver iodide spread throughout a large part of the state of New Mexico and "brought down 394 billion gallons of water" and "that dry creek beds changed into raging rivers."

Consider first the cloud about twenty-five miles away. The following facts can be found in the magazine article and in another paper published on the experiment.[1] Seeding was started at about 5:30 A.M. The cloud base which developed was at about 12,000 feet M.S.L. when it formed at about 8:30 A.M. The cloud grew at the top at the rate of 160 feet per minute and reached 26,000 feet and a temperature of −23° centigrade at 9:57 A.M. Then the cloud shot up at a rate of 1200 feet per minute until it reached 44,000 feet and a temperature of −65° centigrade at 10:12. Rain started from the cloud at 10:06.

Now, the silver iodide was released at the *ground* twenty-five miles away. To be effective, it must have risen in the cloud until it reached a temperature of −5° centigrade at a height of 19,000 feet M.S.L. Albuquerque is at an elevation of approximately 5,000 feet M.S.L., so the silver iodide must have ascended 7,000 feet to reach the base of the cloud and about 7,000 feet more to reach the temperature at which it becomes effective.

The environment of a building cumulus cloud is generally sinking, so the silver iodide must have traveled along the surface. The surface winds were ten miles per hour toward the cloud and, if the silver iodide actually reached the area below the cloud (which it may not have done), it would require two and one-half hours to travel the distance. The silver iodide might then have been under the cloud at 8:00 A.M. To ascend 7,000 feet to the base of the cloud in two hours would require an average vertical velocity of 58

* On May 23, 1950, the Section of Oceanography and Meteorology held a symposium, presided over by A. F. Merewether of Pan-American Airlines, on "Induced Precipitation." This paper was the fourth of four presented at the symposium.

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feet per minute or 29 centimeters per second.

The vertical velocity beneath a cumulus cloud in the building state is very small at the surface and increases with elevation. Byers and Braham[2] have measured these velocities in typical building cumuli, and they found a vertical velocity of about 120 feet per minute at 7,000 feet. From the surface to 7,000 feet, however, the vertical velocity is certainly less than this value. If the vertical velocity were to increase linearly from the surface to 7,000 feet and if it had a value of 120 feet per minute at 7,000 feet, the silver iodide would just barely reach the base of the cloud at 10:00 A.M. The silver iodide must still ascend 7,000 feet more before reaching a temperature of −5° centigrade. Byers and Braham[2] have shown that the vertical velocities increase with height within a building cumulus. The top of the cloud was rising at the rate of 160 feet per minute during this stage of the development, and, if it is assumed that this value of the vertical velocity existed from the base of the cloud to the level at which the temperature was −5°C, forty more minutes would still be required for the silver iodide to reach a level where it could become effective.

Turbulent upward diffusion in the lower levels has been neglected and may modify the results slightly. Also, the silver iodide was permitted to rise for one-half hour before the cloud actually formed. These calculations indicate, however, that the cloud should not have been triggered into a thunderstorm until 10:40 by the silver iodide. Therefore, a doubt arises as to the claim that the silver iodide actually triggered the storm.

The cloud had reached the level of 26,000 feet and a temperature of −23° centigrade when it suddenly spurted upward and developed into a thunderstorm. At such temperatures, almost any natural sublimation nuclei could have triggered the cloud. Before it can be proved that silver iodide released from the ground actually caused a thunderstorm, the path followed by the silver iodide must be accurately traced and it must be proved that the silver iodide had reached a point in the atmosphere where the properties of the silver iodide became effective.

As for the 394 billion gallons and the dry creek beds changed into raging rivers, a cold front which could be located to the north of Albuquerque two or three days before the seeding experiment took place moved steadily through New Mexico and passed Albuquerque at the time the thunderstorm started. All of the rainfall can be explained by the activity of the cold front. Finally, a typical occurrence in New Mexico is a so-called "flash flood," and the dry creek beds are there because the flash floods have produced them.

The rainmaking attempts at the New York watershed will now be considered. Dr. Howell should be highly complimented for his conservative evaluation of the results of his experiments. A rigorous statistical study must be made after the experiments have been completed in order to discover if the hoped for 5 per cent increase over what would have been the natural rainfall without seeding is real or illusory.

For one seeding experiment which has been conducted, however, it will be shown that the silver iodide could not possibly have produced any rainfall. The experiment took place on April 25 and April 26th. Silver iodide was released at the surface at Stroudsburg. Representative atmospheric soundings at Albany show that there was a 6°C temperature inversion over New York State during the time that the silver iodide was being released. The air into which the silver iodide was released was therefore capped by an inversion, and the lapse rates were such that it would be impossible for the silver iodide to penetrate to the freezing level where it becomes effective. In fact, the silver iodide could not possibly have risen above four thousand feet, which was the height of the base of the inversion.

Meteorology is a science which demands careful consideration of many complex factors. Suppose, for example, that one

wanted to increase the rainfall at New York City from a warm front situation. It would be futile to release silver iodide in New York State during warm front rainfall, because the silver iodide could not rise through the cold stable underlying air to the freezing level above in the more moist and potentially warmer air.

Consider a warm front in which the frontal surface slopes upward toward the north and rises one mile over a horizontal distance of 100 miles. For simplicity, let us assume that the winds in the warm air are tangent to the sloping frontal surface and that they have a velocity of 40 miles per hour. Let the freezing level be at 10,000 feet, or approximately 2 miles. If the silver iodide is released at the surface, it must be released at a point 200 miles south of the desired location of the rainfall. It would reach the area where the rainfall was desired about 5 hours after its release at the surface.

Actual weather situations are more complex than the simple model just described. The silver iodide would probably have to be released at the surface in Arkansas or Missouri before it could affect New York State during a warm front regime. Dr. Helmut Weickmann, in a recent seminar at New York University, showed by some theoretical calculations that doubling the number of available nuclei during a warm front rain might possibly increase the rainfall by 5 per cent during light rains, but too many nuclei might actually cause less rain.

In view of the points which have just been considered, it can be concluded that the large scale control of the weather is still only a distant future possibility, that the global nature of the weather has been completely neglected in most of the work which has been done, and that in no case has it been definitely proved that economically significant amounts of rainfall have been produced.

References

1. Langmuir, I. 1949. Progress in Cloud Modification by Project Cirrus. Part I. The control of precipitation from cumulus clouds by employment of various seeding techniques. General Electric Research Laboratory, Schenectady, New York.
2. Byers, H. R. & R. R. Braham, Jr. 1949. Thunderstorm structure and circulation. J. Meteorol., 6(3): 71–86.

DIVISION OF MYCOLOGY

THE OCCURRENCE AND INDUCTION OF MUTATIONS IN THE FUNGI*

By EDWARD J. BACKUS†

The occurrence of mutations in microorganisms has been recognized for a great many years. Those who have handled filamentous fungi are familiar with the sectoring phenomenon so frequently displayed when single cell colonies are grown on an agar plate. Although there was once reluctance on the part of some investigators to consider these variants to be true mutants, it is now well established that sectors arise when mutations occur in one of the cells of the developing colony. As such a mutated cell proliferates into a mass of "abnormal" cells, the typical wedge-shaped sector becomes apparent. While gross morphological mutation has been demonstrated in practically all classes of the fungi, the frequency with which it occurs varies substantially among the different genera and species comprising these groups. Equally common in occurrence are the mutant forms which, while resembling the parent type both structurally and culturally, show major differences in biochemical and physiological behavior.

The problems created by this natural tendency for fungi to mutate fall heavily upon those concerned with taxonomic considerations, especially in the large, diverse groups such as the Fusaria, Penicillia, Asper-

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, May 26, 1950.
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gilli, or the Actinomycetes. The separation of species, or even genera, in the Actinomycetes, is often based upon differences so small that they may be bridged by the deviations occasioned by mutation. The widespread and varied positions which fungi occupy in man's present-day scheme of living have caused the incidence of mutant strains to become important economic factors. For example, the plant breeder and the agriculturist must fear the sudden appearance of "races" of plant pathogens with altered pathogenicity potentials which may be capable of attacking supposedly resistant varieties of economic crop plants. Likewise in the fermentation industries, where so much is based upon an organism's capacities for producing small quantities of certain metabolic products, constant guard must be kept against mutants of lesser capacity dominating the master culture.

Observations that the rate of appearance of mutants in the fungi, particularly in culture on artificial media in the laboratory, was influenced by such environmental factors as temperature, light, pH, accumulation of staling products, salt concentrations, and medium constituents led to organized plans on the part of many investigators to apply variations of these and other conditions in an attempt to cause greater numbers of mutations to occur. The main phase of this discussion is concerned with the progress that has been made in the study of this problem of the induction of mutations in the fungi. Historically, this work goes back about forty years, but it is only within the last decade that microorganisms have become favorite subjects for the study of mutation and inheritance. That they should ultimately have attained this position is not surprising when the tremendous populations that may be worked with, the rapid turnover of generations, and the facility provided for rigidly controlling conditions for all members of a population are compared with the much slower generation time and greater difficulty involved in controlling conditions uniformly in the higher plants and in animals.

The agencies found to be useful in the stimulation of organisms to increased rates of mutation fall chiefly into two categories, radiations and chemical substances. Before proceeding to a consideration of the radiation work, it may be well to point out that the original observations on the mutagenic potentialities of many of the agencies have been made in connection with studies on *Drosophila* or other higher organisms and only later applied to microbiological investigations. Thus, the discovery by Muller in 1927, that mutants can be produced artificially in *Drosophila* by X-ray treatments, and his observation that the induced mutation rate was far greater than the natural rate of mutation in the organism provided the impetus for the tremendous volume of related X-ray work which has followed. It is impossible to point to any single investigation which supplied a similar stimulus for the use of ultraviolet light as a means of inducing mutations. Although the lethal action of ultraviolet on bacterial cells was clearly established as early as 1901 by Bie, the fact that some of the organisms surviving such treatments are mutated was not recognized for many years. The failure of this agency to receive the attention it deserved may be due in part to its comparative ineffectiveness in much of the early induced mutation work on higher plants, a situation undoubtedly resulting from the absorption of the ultraviolet rays by somatic tissues intervening between the light source and the germ cells.

After working out a method with Claus in 1936 whereby each bacterial cell, yeast cell, or fungus spore could be exposed to approximately equivalent amounts of monochromatic radiation, Hollaender collaborated with Emmons in 1939 in a study of the induction of mutations by ultraviolet radiation in the dermatophyte *Trichophyton mentagrophytes*. This was the first major attempt to correlate the effects of wave length and energy absorbed per spore with the rate of mutation. This and subsequent investigations have demonstrated that an ultraviolet wave length of 2650 Å is both

the most effective in killing fungus spores and the most efficient in the induction of mutations. Wave lengths of 2280 Å produce high killing rates and low mutation rates, a situation possibly related to a protective action in the fungi associated with the high continuous absorption of rays in the 2200 Å region by the chitin in the spore walls. Wave lengths of 2967 Å give low rates of both mutation and killing, while those longer than 3400 Å give rise to no genetical variants. It has been suggested that the absorption of ultraviolet light by the nucleoproteins of the chromosomes possibly leads to the genetic effects recorded, the nucleic acids, which show a high absorption coefficient at the 2650 Å wave length, being the active agents in the process. Hollaender and Emmons observed that the rate of mutation increased with energy absorbed per spore (that is, dosage or time of exposure at a constant wave length) up to a certain maximum level and then decreased with further increase in energy. Although this response has not been observed in a few of the ultraviolet studies of mutation in the fungi, the majority of subsequent investigations have been substantiating in nature. Hollaender and his collaborators have indicated that even higher rates of mutation can be attained by the incubation of irradiated spores in physiological salt solution for a few days before plating out on agar. Ultraviolet radiation may cause many organisms to display aberrant forms, sometimes designated as "degenerate phenotypes," which are characterized by a tendency either to die out or quickly to revert to normal after a few generations. It is thought that this type probably is not the expression of any genetic change, but rather is due to a cytoplasmic effect.

Application to the fungi of the principle of inducing mutations by X rays did not assume major importance until it was used on *Neurospora crassa* by Lindegren and Lindegren in 1941, and even then only small attention was given to correlation of dosage to rates of mutation. More concrete data on the method was provided by later investigations, such as that of Sansome, Demerec, and Hollaender on *Neurospora* in 1945, which indicates that the rate of mutation with X-ray exposure increases with dosage in a linear manner and is higher at any given dosage when the radiation is applied in high intensity (5,400 r./min.) than when the same dosage is given in low intensity (240 r./min.). It has been established that X rays cause a great many actual chromosomal aberrations and rearrangements. Changes in an organism which may be attributed to such causes cannot, if one adheres to the strictest terms of the definition, be considered true mutations. Most investigators include these variants among the mutant forms, however, since it is frequently not feasible to detect the actual causes for the modifications observed.

A wide variety of other radiations have been used to stimulate mutation rates in the fungi, including among others cathode rays, gamma rays, neutron bombardment, and uranyl nitrate. In general, results achieved with these agencies have been positive but with rates of induced mutation falling far below those attainable with X ray or ultraviolet ray. While near infrared radiation alone exerts no recognizable effect on mutation rates or chromosome rearrangements, Hollaender and Swanson have demonstrated with *Aspergillus terreus* that a pretreatment with near infrared at 6,000–20,000 Å will significantly increase the rate of X ray-induced mutations as compared to that of X-ray controls.

Practical application of the capacity of radiations to induce mutations in microorganisms has been made to further the advancement of other fields of biological investigation. The fact that mutant strains of an organism may show varied biochemical activities has led to an extensive study of such forms in relation to metabolism, particularly in *Neurospora* and *Penicillium*, with a resultant elucidation of a number of steps in the synthesis of various vitamins and amino acids. Most active investiga-

tors in this field have been Beadle, Tatum, Bonner, Horowitz, Mitchell, and their many associates. Beadle postulates that if proper conditions of experimentation and testing can be set up, the probability is high of obtaining mutations concerned with almost any synthetic process in an organism. Deficiency mutants with altered nutritional requirements have proven to be highly useful as assay organisms for such substances as *p*-aminobenzoic acid, choline, and leucine, growth responses being comparatively proportional to the amount of the factor present in the material being tested. The induction of mutations by radiation treatments has played a major role in the development of antibiotics produced by microorganisms. Outstanding in this respect is the genealogy of *Penicillium chrysogenum* in relation to penicillin production, with the major successes being recorded in the development of X-1612 at the Carnegie Institution of Washington, Biological Laboratory, Cold Spring Harbor, N. Y. by X-ray treatment of conidia of NRRL 1951-B25, and the subsequent isolation of mutant strain Q176 at the University of Wisconsin following ultraviolet treatment of conidia of X-1612. No doubt major improvements have been made in the organisms producing streptomycin and chloromycetin by induced mutation studies in the industrial laboratories. I can report from personal experience that ultraviolet radiation has been used to good advantage in our laboratories in the induction of mutants of *Streptomyces aureofaciens* capable of producing quantities of aureomycin well in excess of the capacities of the original soil isolate.

Historically, the search for chemical mutagens is very old. Even after the demonstration of the effects of radiations on mutation rates, the quest for chemical agencies was not abandoned, since it is generally believed that mutation is a chemical process. Therefore, the possibility exists that a known chemical substance might provide a better lead as to the nature of the gene itself. The geneticists' regard of the radiations as too drastic and indiscriminate in their action supplied added stimulus to this search. Probably the greatest hazard attendant upon the use of chemicals to induce mutation is that of achieving penetration deep enough to reach the chromatic materials of the cells without having to use concentrations that are so strong that excessive killing results.

While the first well-organized approach to the subject in the fungi was made in 1939–40 by Steinberg and Thom when they attempted to induce mutations in *Aspergillus niger* by altering gene proteins with a variety of chemical agents, the most effective of which proved to be $NaNO_2$, the subsequent demonstration in *Drosophila* by Auerbach and Robson that mustard gas is a powerful mutagenic agent provided the real impetus for the development of this field. Soon included in the scope of their work was a series of chemically related compounds, sulfur mustard and the nitrogen mustards. All of these substances are strong vesicants and all proved to be excellent agents for the induction of mutations in *Drosophila*.

Confirmatory evidence of the efficiency of mustards as mutagens for microorganisms has accumulated rapidly in the last four years. Mutation rates in *Neurospora crassa* treated with nitrogen mustard were found to be similar to those obtained by ultraviolet radiation, and even higher rates were reported when germinating spores were treated. The data of Tatum and of Giles suggests that the types of mutants induced in *Neurospora* by the various mustards are both quite similar to each other and to the ultraviolet derived mutants, those characterized by an inability to synthesize methionine being the most frequently occurring class of biochemical variants appearing as a result of treatments with any of these agencies. Other microorganisms which have responded to nitrogen mustard treatments with increased mutation rates include *P. notatum*, *Coprinus fimetarius* *Aspergillus nidulans*, and *E. coli*.

The understanding of the mode of action of the mustards has not advanced far be-

yond the hypothesis stage. A similarity between the genetic effects of the mustards and of X rays has been suggested by the *Drosophila* studies, the main points of difference being in the greater number of chromosomal rearrangements occurring in the irradiated material and the apparently delayed mutation which may result following mustard treatment. Since it has been chemically demonstrated that the mustards react with various amino acids, such as arginine, methionine, and threonine, it is reasonable to suppose that these highly reactive substances, with their unsaturated nitrogen or sulfur atoms, may possibly combine with the proteins of the genetic materials of the cell and alter them in such a manner as to cause a change in the subsequent manifestations of the organism.

So long and diverse is the list of chemicals which have been tried as mutagenic agents in recent years, that we must limit our attentions here to a brief mention of those which have produced positive reactions in tests on microorganisms. The alkaloid, colchicine, so widely used in higher plants to produce polyploid forms, has been used to induce "giant" races in *P. notatum* but generally has enjoyed very limited success and usage in mycological work. Included in the list of substances shown to induce at least slight increases in mutation rates in microorganisms are camphor, allyl isothiocyanate, various carbamates, benzimidazole, acriflavine, theophylline, and caffeine. Very recently, Demerec has shown that a variety of carcinogens belonging either to the polycyclic hydrocarbons or to the azo- compounds are capable of inducing mutation in *Drosophila*. Little is yet known concerning the reaction of fungi to these agencies. The scattered favorable reports of the use of miscellaneous chemical compounds to induce mutations in the fungi serve as the basis for continued interest in further exploration of the field.

Recent years have seen a growing interest in combination treatments to induce mutation, although their use has been almost exclusively limited to animal and higher plant studies. However, Swanson and Goodgal have demonstrated that weak, nonmutagenic doses of nitrogen mustard followed by a normal ultraviolet radiation will markedly increase mutation rates in *A. terreus* over and above that obtainable with the radiation treatment alone.

Stone and his associates have conducted a series of experiments with *Staphylococcus aureus* which have shown that pretreatment of media with ultraviolet light induces marked increases in the rate of mutation in organisms subsequently grown upon it. Similar results were recorded when 1 to 6 ppm. of hydrogen peroxide were added to the medium in place of the irradiation treatment. More recently, the method has been applied to *Neurospora crassa* and extended to include a series of organic peroxides, all of which show mutagenic potentialities. It has long been known that ultraviolet radiation of water produces considerable quantities of hydrogen peroxide. This recent work adds evidence in support of the hypothesis that ultraviolet light possibly increases mutation rates because of the formation of peroxide compounds in media or cells subjected to radiation.

It is hoped that his brief review has emphasized the great progress that has been made in the induction of mutations during the last fifteen years. This study is now a vitally active, growing field of scientific endeavor. Although an absolute comprehension of means by which the various agencies bring about changes in the genetic materials of the organisms is not possible at this time, working theories and hypotheses have been advanced, and real progress toward this understanding is being made.

References

1. Auerbach, C. 1949. Chemical mutagenisis. Biol. Rev. **24:** 355–391.
2. Auerbach, C. & J. M. Robson. 1946. Chemical production of mutations. Nature, Lond. **157:** 302.
3. Beadle, G. W. & E. L. Tatum. 1945. Neurospora. II. Methods of producing and detecting mutations concerned with nutritional requirements. Am. J. Bot. **32:** 678–686.

4. BONNER, D. 1946. Biochemical mutations in *Neurospora*. Cold Spr. Harb. Symp. Quant. Biol. **11:** 14–24.
5. HOLLAENDER, A. & C. W. EMMONS. 1941. Wavelength dependence of mutation production in the ultraviolet with special emphasis on fungi. Cold Spr. Harb. Symp. Quant. Biol. **9:** 179–186.
6. HOLLAENDER, A., E. R. SANSOME, E. ZIMMER, & M. DEMEREC. 1945. Quantitative irradiation experiments with *Neurospora crassa*. II. Ultra-violet irradiation. Am. J. Bot. **32:** 226–235.
7. HOLLAENDER, A., K. B. RAPER, & R. D. COGHILL. 1945. The production and characterization of ultraviolet-induced mutations in *Aspergillus terreus*. I. Production of the mutations. Am. J. Bot. **32:** 160–165.
8. HOLLAENDER, A. & C. W. EMMONS. 1946. Induced mutations and speciation in fungi. Cold Spr. Harb. Symp. Quant. Biol. **11:** 78–84.
9. LINDEGREN, C. C. & A. LINDEGREN. 1941. X-ray and ultraviolet induced mutations in *Neurospora*. J. Heredity **32:** 405–412, 435–440.
10. SANSOME, E. R., M. DEMEREC, & A. HOLLAENDER. 1945. Quantitative irradiation experiments with *Neurospora crassa*. I. Experiments with X-rays. Am. J. Bot. **32:** 218–226.
11. SWANSON, C. P. & S. H. GOODGAL. 1948. The effects of nitrogen-mustard on the ultraviolet-induced mutation rate in *Aspergillus terreus*. Genetics **33:** 127–130.
12. WYSS, O., J. B. CLARK, F. HAAS, & W. STONE. 1948. The role of peroxide in the biological effects of irradiated broth. J. Bact. **56:** 51–57.

OPENING AND DEDICATION OF THE ACADEMY BUILDING

On April 11, 1950, The New York Academy of Sciences officially opened its new home at 2 East Sixty-third Street with a formal reception marking the beginning of the celebration for The Academy's Members and their Guests. The dedicatory program was as follows: President Crossley gave the address of welcome; Doctor Detlev W. Bronk, President of John Hopkins University, delivered an address emphasizing the role of The New York Academy of Sciences as a forum for the presentation and discussion of scientific ideas. Doctor George B. Pegram, Vice-President of Columbia University and of the Academy, dedicated the new building.

The Academy held open house with reception, from three to seven o'clock, on each of the four days from April 11th to April 14th, for the Members and their Guests. Attendance for the period totaled 2,592. A special feature of the House Opening Celebration was an exhibition of paintings and sculpture by Joel Stolper, well-known animal painter; an exhibit of photographs of famous scientists by Doctors Harden F. Taylor, J. A. Aeschlimann, and M. F. Furter; as well as two flower paintings by Sidney Hatchell, of Mystic, Connecticut. The gardens and floral decorations were arranged by Mr. Hatchell. Music was rendered by an orchestra under the direction of Jerry Lloyd.

The Address by President M. L. Crossley follows.

ADDRESS OF WELCOME

By PRESIDENT M. L. CROSSLEY

Great occasions, like great deeds, result from persistent endeavor. This is such an occasion; the culmination of a long struggle to secure a home for the Academy. Faith, hope, and good works, welded into an effective force by the gracious, persuasive, dynamic personality and indomitable will of the Executive Director, Mrs. Eunice Thomas Miner, have finally achieved success and brought us to this historic moment of jubilation, which we wish to have you share with us.

We welcome you to our party; we are glad you could come. You will see what we have now and think of its potential possibilities. Some of you will be privileged to participate in the scientific work of the Academy and will enjoy the service. Others will have the satisfaction of knowing that their contributions to the physical resources of the institution will have helped, in time, to establish the greatest scientific center in the world.

We are grateful to all who have helped us to realize our aims thus far. We hope to have their continued interest and help—both members and friends. Particularly do we, at this time, acknowledge our great debt of gratitude to the Woolworths for their generosity and splendid foresight in making this building available to the Academy for a scientific center in New York. Also, we are not unmindful of the debt we owe to all who have served the Academy so well throughout the long years of its past history and who have made this occasion possible. Their contributions are fused into the very life of the institution and help to guide its future course of action. To the Officers of the Council, Committees, and Administrative Staff; to the architect, Mr. W. Stuart Thompson, and the other friends whose counsel and aid have been so generously given, we express our deep appreciation and offer sincere thanks.

One hundred and thirty-three years is a long time in the life of this nation. On February twenty-fourth last, the Academy attained this ripe old age, but without any signs of senility. On the contrary, it is a vigorous, active, purposeful institution with great capacity for service to the sciences. Its plan of action enables investigators of the different phases of a problem to report their results and to confer with one another regarding the significance of their findings. These reports, when thus evaluated and published in the Annals of the Academy, serve as full and authoritative records of what has been accomplished. The conferences, under expert leadership, endeavor to cover each subject as fully as possible, giving the scientific investigators in the field the opportunity to present and evaluate critically all the information available, so that the knowledge resulting will be a full and complete unit for use of other interested investigators. This purpose is steadfastly maintained. Solidarity in action is dependent upon oneness of purpose. This the Academy has.

Individual fragments of knowledge, like gold dust in alluvial soil, have no value until assembled, correlated, and evaluated. The conference plan permits the Academy to seek those who have fragments of experimental evidence and gems of thought which they can fit into a composite picture to reveal, not only the true significance of what has been accomplished, but, what is usually equally important, the missing fragments needed to give the picture reality and value. There are many facets to a scientific gem of knowledge. The most important of these are seldom obtained by direct application of wholly new concepts. What appears to be new is generally a composite of the old and the new, with never before recognized implications and challenging significance so striking as to obscure its true symbiotic nature. Such knowledge more often lies at the apex of a pyramid whose base and sides are composed of an intricate mosaic of supporting facts derived from the information secured as a result of many different types of investigations, touching on various phases of the problem. The interfusion of related facts leads to the knowledge required to chart the course to follow for new discoveries.

The role of the Academy in helping to see that the results of scientific investigations become available in the world of affairs can go on now with accelerated speed. Better facilities will enhance the service rendered by the many scientists who labor willingly and loyally for this institution and the public it serves. Buildings and equipment are only auxiliary to this service, however, and, in the main, the work of the Academy, as in the past, will be measured in terms of the caliber of the people who serve it. The greater the achievements of science and technology, the more need there is for vision and competence in those who recognize and interpret scientific results.

Any appraisal of what lies ahead in opportunities for the Academy must be based on an understanding and appreciation of its work in the past and the relation of this to the advancements in the sciences. Much of great significance has happened in

science in the United States and in the entire world during the life span of the Academy. The combined powers of science and technology, aided by industry, have brought into being materials and things which have greatly improved human well-being. Basic to this accomplishment have been the discoveries of the essential features of atomic and molecular architecture responsible for the properties of matter; the nature of disease and its causative agents; the essential biochemistry involved in many of life's processes, including glimpses into the marvelously interesting and complicated steps controlled by specific chemical substances such as enzymes and hormones produced by definite biological structures when needed; genes and their role in heredity; the chemotherapy of infectious diseases, with synthetic drugs and antibiotics saving many from untimely death; the causative agents in certain cancers and the transplantation of cancers in animals, with accumulating knowledge of their common properties and requirements, as well as the conditions required for their growth; the growth of cancer tissue outside of the body in artificial media and the transformation of normal cells into cancer cells which can then be implanted into animals, survive, and grow resembling types of spontaneous cancer; the mutation of plant and animal cells; and the realization of the alchemist's dream of the transmutation of the elements. These and many more equally important discoveries have resulted from the work of scientists in many different laboratories in many countries and have added to the common store of knowledge available to all mankind.

The Academy has been concerned with many of these developments in the past and will continue to play an important role with such progress in the future. In addition, it must now assume the leadership in focussing attention on the scientific principles governing man's behavior in his adjustment to a changing environment. The chemical and physical changes resulting in response to physiological stresses due to the impact of environmental conditions and the effect of such changes on emotional and mental states, with the resulting behavior patterns, must be understood and interpreted, so as to help man in shaping his judgments that he may act with wisdom and tolerance.

There is much information scattered among the scientific publications on special bits of evidence from animal experimentation and some from clinical observations of nervous disorders and insanity in man that, when correlated and evaluated, may furnish valuable data which would serve as a guide to future study of the problem. In our anxiety over the physical ills that beset man, we are prone to overlook the mental derangements which are both costly and dangerous for the body politic.

In spite of the fact that a large percentage of all the hospital beds in this country are occupied by mental cases, the continued inhuman treatment of many of these unfortunates reduces them to the status of lost souls. I hope that the Academy will give careful consideration to the sponsoring of a world conference here in New York next year, perhaps at the time of the meeting of the Twelfth International Congress of Chemistry, at which the essential experimental data bearing on this subject may be presented and evaluated.

Much stress has been placed on the contributions of science to the material progress of mankind. However, it is not what a man *has* but what he *is* that determines his behavior in society. No greater truth has ever been spoken than, "man does not live by bread alone." Its significance is ageless and its application universal. Material possessions in themselves have no power to satisfy man's aspirations and keep ever before him that vision of greatness essential to full and considerate living.

The knowledge and the products which result from the achievements of science and technology do not, in themselves, assure fullness of life or prosperity. This depends upon their proper use. They may be employed for the good of society or they may

be used as tools and instruments of destruction. What one does with what he has depends upon his sense of spiritual and moral values and not on scientific laws and technological skill. However, science can not take refuge behind the statement that its function is only to provide the knowledge by which the material things of life may be made available. It must share the responsibility with those who use the knowledge for what is done with that which it creates. Maximum human good must result from the interfusion of ideals and practical achievements.

Some critics blame science and technology for most of the ills of modern society, seeing in the products of the scientific age the means for destroying the moral fiber of man and converting him to an irresponsible, selfish, cruel, intolerant being, fostering wars which will ultimately destroy our civilization. This is nonsense. The misuse of the good things of life, regardless of how they are provided, results from certain defects in human character. It stems from selfishness, suspicion, and jealousy, which are not the result of scientific discoveries or the technological developments of these. They have always played tragic roles in the history of mankind.

It is pertinent to inquire if the methods of science could not be applied to the improvement of human relationships. Science seeks facts, arranges them in proper order, evaluates the results without prejudice, and interprets their significance in relation to the conditions surrounding the problem. What catastrophes might be avoided in the future if the scientific method were applied to the solution of international problems!

Much has been accomplished by science in penetrating to the core of the atom and to the heart of the gene. The knowledge gained should tend to create a better world —one in which all peoples can live in peace and harmony. The power derived from atomic energy should enhance social and economic progress, if we would talk less about its devastating potentialities in war and concern ourselves more with its development for peaceful pursuits. The will for peace is the only sure power factor that can prevent war and its consequences.

NEW MEMBERS

Elected May 25, 1950

SUSTAINING MEMBERSHIP

Beherens, Herbert C., Ph.D., Physics and Geology. Chief Engineer, Diebold Incorporated, Canton, Ohio.

Belock, Harry D., P.E., Engineering. Executive Vice-President, General Manager, Reeves Instrument Corporation, New York, N. Y.

Draper, Stark, D.Sc., Engineering. Director, Instrumentation Laboratory, Massachusetts, Institute of Technology, Cambridge, Mass.

Guillemin, Ernst., Ph.D., Engineering. Professor, Massachusetts Institute of Technology, Cambridge, Mass.

Liu, Y. J., D.Sc., Engineering. Staff, Instrumentation Laboratory, Massachusetts Institute of Technology, Cambridge, Mass.

Liebler, John B., M.D., Internal Medicine, Resident Physician, Halloran V.A., Hospital, Staten Island, N. Y.

Locke, Arthur Preston, Ph.D., Physics, Chemistry, Biology. Research Director, Zonite Products Corp.

Luthy, Max, D.SC., Organic Chemistry. Plant Manager, The Givaudan Corp., Delawanna, N. J.

Rubin, I. C., M.D., Gynecological Physiology. Clinical Professor, New York University Medical College, New York, N. Y.

ACTIVE MEMBERSHIP

Achilles, Edith M., Ph.D., Psychology. Vocational Guidance, New York, N. Y.

Alter, Emil N., M.A., Chemistry. Research Assistant, Mount Sinai Hospital, New York, N. Y.

Alture-Werber, Erna, Ph.D., Microbiology. Chief, Research Laboratory, Jewish Hospital, Brooklyn, N. Y.

Benz, Edward John, M.D., Pathology. Fellow in Pathology, Mayo Clinic, Rochester, Minn.

Berg, Frantz F., Ph.C., Chemistry. Associate Director, Pharmaceutical Application, Calco Chemical Division, American Cyanamid Company, Bound Brook, N. J.

Bogert, Marston T., LL.D., Organic Chemistry. Professor Emeritus, Columbia University, New York, N. Y.

Cumings, Lawrence, D.S.C., Foot Health. Director, Community Health Service, National Association of Chiropodist, Norwich, N. Y.

Diringer, David, L.L.D., Archeology. Lecturer in Semitic Epigraphy, University of Cambridge, Cambridge, England.

Eakin, Alexander. Associate, Brown, Crosby and Company, New York, N. Y.

Elhat, Blanche M., B.A., Psychosomatics, Bacteriology. Formerly Laboratory Instructor, Adelphi College, Garden City, New York.

Finkelstein, Hyman, M.D., Public Health. Physician in Charge, Social Hygiene Clinic, Department of Health, New York, N. Y.

Gardner, Thomas S., Ph.D., Gerontology. Senior Research Chemist, Hoffman-La Roche Inc., Nutley, N. J.

Gerber, Milton M., B.E.E., Electrical Engineering. Electrical Engineer, Reeves Instrument Corporation, New York, N. Y.

Gray, Edward T., B.Dc., Geology, Anthropology. President, Non-Chemical Water Condition of New York, New York, N. Y.

Greiner, Theodore H., M.D., Cardio-vascular Pharmacology. Research Fellow, Washington University, St. Louis, Mo.

Grosslight, Charles L., B.Sc., Biology, Chemistry. Research Chemist. New York, N. Y.

Hamburg, Murray, D.D.S., Dentistry, Bio-Chemistry. Clinical Dentist, Halloran Veterans Hospital, Staten Island, N. Y.

Heiser, Bruce A., B.A., Organic Chemistry. Research Chemist, Calco Chemical Division, American Cyanamid Company, Bound Brook, New Jersey.

Hodge, G. B., M.D., Surgery. Surgeon, General Hospital, Spartanburg, S. C.

Korvin-Kroukovsky, Boris V., M.S., Dynamics. Research Professor, Stevens Institute of Technology, Hoboken, New Jersey.

Lasker, Edward, E. E., High Frequency Heating Devices, Automatic Calculating Machines. Consultant to manufacturers.

Lasser, Richard P., M.D., Cardiovascular Physiology. Fellow in Medicine, Mt. Sinai Hospital, New York, N. Y.

Laufer, Louis, M.S., Organic Compounds. Chief Research Chemist, Schwarz Laboratories Inc., Mt. Vernon, N. Y.

Lauffer, Paul G., Ph.D., Organic Chemistry, Biochemistry. Chief Chemist, The George W. Luft Company, Long Island City, N. Y.

Long, Louis, Jr., Ph.D., Organic Chemistry. Managing Director, Owner, Concord Laboratories, Cambridge, Mass.

Lowenstein, Marta G., Ph.D., Biology. New York, N. Y.

Magariello, Edward R., M.S., Organic Chemistry. Research Chemist, National Dairy Research Laboratories, Oakdale, L. I., N. Y.

Malvin, Harry H., B.A., Chemistry. Laboratory Technician Student, New York University, New York, N. Y.

Marshall, Theodore H., Ch.E., Chemical Engineering. Manager of Manufacturing, Lederle Laboratories Division, American Cyanamid Company, Pear River, N. Y.

Martin, Alice J., B.A., Bacteriology, Mycology. Bacteriologist, Health Department, Nassau County, N. Y.

Marx, Hal M., B.S., Chemistry. Chemist, Medical Center, Jersey City, N. J.

Masci, J. N., P.C.P.S., Pharmaceutical Research, Director of Pharmaceutical Research Section, Johnson and Johnson, New Brunswick, N. J.

McGuinness, Doris J., B.S., Hematology. Technician, Merck Inst., Rahway, N. J.

Mindlin, Henry J., M.D., Gynecology, Obstetrics. Staff, Kew Gardens General Hospital, Kew Gardens, N. Y.

Mishler, Jay E., M.D., Eye Specialist. Private Practice, Atlantic City, N. J.

Newman, Lloyd H., M.D., Biochemistry. Head, Department of Chemistry, Howard University, School of Medicine, Washington, D. C.

Papakonstantinou, Ilias, M.D., Medicine. Resident, N. Y. Orthopedic Hospital, New York, N. Y.

Plunkett, John E., F.R.C.P., Internal Medicine. Senior Attending Physician, Civic Hospital, Ottawa, Canada.

Pritham, Gordon H., Ph.D., Physiological Chemistry. Professor of Physiological Chemistry, The Pennsylvania State College, Pennsylvania.

Reisfield, Donald R., M.S., Endrocrinology. Research Associate, Columbia University, New York, N. Y.

Ronkin, R. R., Ph.D., Biology. Assist. Prof. of Physiology, University of Delaware, Newark, Del.

Rubenstein, Robert, M.D., Medicine. Senior Assistant Attending Physician, Medical Service, Christ Hospital, Jersey City, N. J.

Sanborn, Eldon N., M.S., Biochemistry, Bacteriology. Control Chemist, White Laboratories, Newark, N. J.

Sass, Martin, M.S., Biochemistry. Research Assistant, School of Public Health, Columbia University, New York, N. Y.

Seeley, Donald B., Ph.D., Fermentation. Director, Fermentation Research, Charles Pfizer and Company, Groton, Conn.

Sherover, Max, Psychology, Anthropology. President, Linguaphone Institute, New York, N. Y.

Shore, Seymour M., M.D., Biology. Resident, V.A. Hospital, Bronx, New York.

Shukis, Anthony J., Ph.D., Chemistry. Assistant Director, Laboratories, Dodge and Olcott, Inc., New York, N. Y.

Shull, Gilbert M., Ph.D., Biochemistry. Research Biochemist, Charles Pfizer and Company, Brooklyn, N. Y.

Slobody, Lawrence B., M.D., Pediatrics. Professor, New York Medical College, New York, N. Y.

Soskin, Sophia, M.S., Psychology. Private Practice, Brooklyn, N. Y.

Stynler, Frederic E., Ph.D., Chemistry. Research Associate, Department of Chemistry, Mt. Sinai Hospital, New York, N. Y.

Taylor, M. Wight, Ph.D., Biochemistry. Associate Research Specialist, New Jersey Agricultural Experiment Station, New Brunswick, N. J.

Tennison, Lafayette B., D.V.M., Veterinary Medicine. Veterinarian, Animal Industry Section, Lederle Laboratories Division, American Cynamid Company, Pearl River, N. Y.

Vitenson, Boris, M.D., X Rays. Interne, Metropolitan Hospital, Welfare Island, New York.

Walters, Vladimir, M.S., Biology of Fishes. Assistant, Department of Biology, New York University, New York, N. Y.

Wall, Robert L., M.D., Hematology. Chief, Blood Research Section, Army Medical Center, Washington, D. C.
Weiss, Alter, M.D., Neoplastic Diseases. Associate Physician, Montefiore Hospital, New York, N. Y.
White, John I., B.S., Biochemistry. Research Assistant, Rutgers University, New Brunswick, N. J.
Wilks, Samuel S., Ph.D., Mathematical Statistics. Professor, Princeton University, Princeton, N. J.

STUDENT MEMBERSHIP

Bernstein, Fabian, B.S., Physical Chemistry. Research Fellow, Polytechnic Institute, Brooklyn, N. Y.
Katz, Florine, B.A., Psychology. Student, Psychological Services, Department of Education, The City College of New York, N. Y.
May, Leopold, M.S., Chemistry. Graduate Student, Instructor, Polytechnic Institute of Brooklyn, Brooklyn, N. Y.
Thurlow, Jane O., B.S., Biochemistry. Graduate Student, Bureau of Biological Research, Rutgers University, New Brunswick, N. J.
Weiss, Lawrence, B.A., Micropaleontology. Geologist, U. S. Geological Survey, Ground Water Division, New York, N. Y.

ADDITIONS TO LIFE MEMBERSHIP CLASS BY TRANSFER

Wladimir V. Archawski
Gordon Y. Billard
*Trigant Burrow
J. A. Dawson
Paul de Haen
Robert C. Demsker
Jacob Feld
Clayton W. Ferry
Olive Huber
Eleanor Alexander Jackson
Serge A. Korff
John H. La Ware
Louis W. Maraventano
Joseph Monachino
Boris Pregel
Bret Ratner
H. Christine Reilly
William Hays Smyers
Abraham Taub
Andrew Yeomans

*Deceased